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# *AHA Pamphlets*

## **Essays on the Columbian Encounter**

**edited by Carla Rahn Phillips and David Weber**

This AHA series, presented in celebration of the Columbus Quincentennial, is designed to assist secondary-school educators in introducing the era of Christopher Columbus as one of mutual discovery between radically different cultures.

### **Imagining the Other: First Encounters in North America**

*by James Artell*

The author provides readers with vivid scenes of first encounters among Europeans and Native Americans. Descriptions of the very different customs and cultures, the language barrier, and the conflicting expectations and aims are presented in a highly readable, informative style.

### **Before 1492: Christopher Columbus's Formative Years**

*by William D. Phillips, Jr.*

Calling upon the variety of works written since the fifteenth century on Columbus's life, the author discusses the possible and probable events of the enigmatic explorer's lesser-known early life. Rumor, conjecture, evidence, and myth regarding Columbus's motives and methods are given thoughtful attention. Available spring 1992.

### **The Exploration of North America**

*by James P. Ronda*

In an essay that presents a thoughtful overview of the exploration of North America from the era of Columbus to the present, the author discusses the motivations and agendas of explorers and their backers. These "great men" are depicted in a more believable, realistic vein in an attempt to understand the truth behind many of the heroic myths. Available summer 1992.

### **North America and the Beginnings of European Colonization**

*by Karen Ordahl Kupperman*

In this final essay in the AHA's Essays on the Columbian Encounter, Karen Ordahl Kupperman discusses the strategies, conditions, and repercussions of European colonizers' attempts to settle several regions of North America. Available fall 1992.

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## In This Issue

**Frederic Wakeman, Jr.**'s *Presidential Address* takes us on a tour of the world that begins with his own childhood voyage around Cuba following the route of Columbus and with the Chinese forcibly brought to Cuba in the nineteenth century as indentured laborers. Wakeman convincingly demonstrates that these Chinese were not passive victims (as they were stereotypically portrayed), and he traces their traditions back to their seafaring forebears, who moved in great armadas across the seas to India, Arabia, and Africa. The enduring empires of the Spanish, British, and Chinese that form the backdrop for his story provide a sharp contrast to the brief fifty-year-old hegemony of the United States, now in decline as Asian economic power rises to dominance with the help of millions of overseas Chinese who, scattered by the attractions or impressment of the earlier imperial powers, are now investing billions of dollars annually in modern China.

**Michael D. Bess** examines the career of E. P. Thompson for insights about the relationship between militant political activism and historical scholarship. Thompson continually faced questions about whether a historian could be an activist without compromising scholarly judgment and about the effectiveness of a political partisan who was also a scholar. From the demands of self-censorship in the 1950s when he was a Communist to his efforts to present a balanced critique of both superpowers and their ideological defenders in the 1980s, Thompson had to contend with the tension inherent in his two roles and with critics who were suspicious of his allegiance to the standards required by each role.

The fields of medieval German and Catalan history have not previously been used to illuminate one another, possibly because of the abundant scholarship available on Germany and the limited research on Catalonia. **Paul Freedman** demonstrates the fruitful comparisons that can emerge from a study of the nearly contemporaneous peasant wars of the two countries, especially in regard to the role of religious reform and the formation of peasant ideology. In both cases, participants emphasized Christian liberty, which they opposed to serfdom, a violation of divine law. Elite and popular arguments about serfdom also shared key elements. The justifications for revolt in Catalonia differed from those in Germany in avoiding anti-clerical expressions and programs of religious reform, demonstrating that revolt in the Middle Ages did not have to incorporate radical religious demands.

Specialists on Soviet history have recently begun analyses of the rich political iconography of the period, so important to rulers who had to communicate with people of many different languages and contend with high rates of illiteracy. **Victoria E. Bonnell** looks at the posters associated with the state's drive to collectivize agriculture in the 1930s and examines the changing image of the agrarian woman, first shown as a buxom and backward peasant, then as a slender, progressive collective farmer and tractor driver. Following the "great retreat" of 1934, the female image reincorporated traditional elements, and the visual language began to prefigure the pastoral romance of high Stalinism.

Some of the themes introduced in Frederic Wakeman, Jr.'s *Presidential Address* can be pursued further in the review article by **John E. Wills, Jr.** Wills discusses recent scholarship of maritime Asia, which has shifted from the thematics of European expansion to a concentration on the maritime activity of the indigenous peoples prior to the arrival of the Europeans and to the response of these peoples to European encroachment. Again, in these latest works, Asians are shown not to have been passive victims but active participants in many important developments that are now seen as having emerged from the interaction of Asian and European enterprise.

**John Mack Faragher** finds the legacy of Frederick Jackson Turner still alive among a small group of historians of the American West, who, in arguing for Turner's continued relevance, unfairly (in Faragher's view) represent the work of new western historians as "a picture of unrelieved bleakness." But Faragher also believes that the anti-Turnerians exaggerate the strength of their opponents. The debate, rehearsed in a recent outpouring of books on western history, testifies to the intellectual energy of this rapidly growing field.

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BY FREDERIC WAKEMAN, JR.

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## Voyages

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FREDERIC WAKEMAN, JR.

I WAS BORN ON DECEMBER 12, 1937, the day the Japanese Imperial Army was set loose upon Nanjing for three weeks of rape and slaughter. My first connected memory as a child is of sitting in a sandbox at an apartment complex overlooking New York's George Washington Bridge on a cold winter's afternoon. A window flew open on the fourth floor in the apartment next to ours, and a man—whom I later knew to be William Rogers, then Tom Dewey's assistant district attorney and later Eisenhower's attorney general and Nixon's secretary of state—shouted down to my father, who was watching me play in the sand, "Fred, they've bombed Pearl Harbor!" I know that time telescopes in a child's mind, but it seemed that only a few days later, my father was holding me in his arms, scratchy in a navy lieutenant's dress blues, at Grand Central Station, kissing me goodbye as he left for San Diego and wartime service in the Pacific.

Although the beginning of the American empire in Asia is conventionally marked by the conquest of the Philippines in 1898, the visceral experience of my generation—the generation that came of age in the 1950s—is the American empire that ascended during and after World War II. That age of American military and economic might, which lasted only about fifty years, must be one of the shortest-lived hegemonies in world history.

After the Second World War, my family moved to Cuba, where my father enrolled me in an academy in Havana called Colegio Baldor. It was not easy being one of the few North Americans among Cuban schoolboys so bellicosely proud of their national heritage, and as we would stand at attention on the sweltering parade grounds listening to veterans of 1898 stirringly recall their victories against the Spanish on the plains of Camagüey, I knew that after classes, things might not go so easily with an eleven-year-old Yankee when I ran the older boys' gauntlet away from teachers' eyes outside the school's main gate. It was with a great and liberating sense of relief, then, that I heard my father announce his plans to take me out of school early in 1949 so that the entire family could retrace the second voyage of Columbus on our 56-foot ketch, the *Chalene*.

This trip was not entirely unexpected. My father revered Samuel Eliot Morison, and after he finished Morison's biography of Christopher Columbus, *Admiral of the Ocean Sea*, he passed it on to me with the usual caution that the family would be expecting to hear my dinner-table review of the book within the week. This was a task I sometimes resented, especially when it came to giving book reports on Carlyle, Gibbon, or Spengler. But Morison's personally infused account of the

four voyages of discovery enthralled me, and I read the book several times, lingering over exciting pages such as the description of Columbus's effort, in 1494, to sail along the south shore of what he took to be a large peninsula jutting out from China and the Asian mainland. The "peninsula," of course, was Cuba, where I lived.

Columbus had left that spurious Chinese peninsula behind in November 1492 to sail back to Spain. Returning to Hispaniola on the second voyage, he set sail from Isabela with the *Niña* and two Portuguese-style lateeners on April 24, 1494, intending to navigate the south coast of Cuba "until definite proof of its continental character was obtained, and if possible to make contact with the elusive Grand Khan."<sup>1</sup> Four days later, sailing under steady northeasterly trade winds across the Windward Passage, Columbus reached the southeastern tip of what he believed to be the beginning of the Asiatic mainland.

Four hundred and fifty-five years later, we rounded that same tip of southeastern Cuba in the *Chalene* and followed Columbus's route along the coast of Oriente province past the arid vegetation of the southern slope of the Sierra Maestra. I remembered reading that when his ships had reached Guantánamo Bay, called Puerto Grande by Columbus, the Spaniards had gone ashore and found giant iguana lizards—"the most ugly and disgusting creatures they had ever seen"—being roasted and eaten by the Indians.<sup>2</sup> When we anchored and went ashore in 1949, we found giant iguanas still there, and when we sailed on westward for forty miles, we entered the same narrow barracuda-infested channel into the Bay of Santiago that Columbus had discovered, where the site of an important Indian city named Bagatiquir was chosen by Diego de Velásquez in 1514 as the location of Cuba's second major Spanish urban settlement.<sup>3</sup>

The Spaniards had simply overwhelmed the Indians they encountered along the way. After Columbus turned south from the Gulf of Guacanayabo in strong winds and sailed away from Cuba under bare poles to Jamaica, he reached what he called Santa Gloria and the English later called St. Ann's Bay to spend the night on May 5, 1494. His three vessels that evening drove off a group of Arawak Indians in seventy large war canoes by firing blank salvos at them from their lombards. At Puerto Bueno, Columbus and his men were again attacked by Indians, this time ashore, and they retaliated with crossbows, also setting loose a big dog "who bit them and did them great hurt, for a dog is worth ten men against Indians."<sup>4</sup>

On May 9, 1494, Columbus reached Montego Bay (El Golfo de Buen Tiempo) on the west end of Jamaica and thence turned north to search for a place on the south Cuban coast called "Magón" by the Arawak Indians, which Columbus mistook to be "Mangi," Marco Polo's name for the southern Chinese province of Fujian. Reaching the Cuban coast again, Columbus sailed around the Zapata Peninsula to a shallow bank now called the Jardines. This is where our own

<sup>1</sup> Samuel Eliot Morison, *Admiral of the Ocean Sea: A Life of Christopher Columbus* (Boston, 1942), 445.

<sup>2</sup> Morison, *Admiral of the Ocean Sea*, 449.

<sup>3</sup> Morison, *Admiral of the Ocean Sea*, 451.

<sup>4</sup> Morison, *Admiral of the Ocean Sea*, 452–53.

*Chalene* foolhardily followed the log of Columbus, choosing to ignore Morison's vivid warning about these waters:

The Admiral had boldly sailed into a tangled archipelago, the cays off the Zapata Peninsula, which are difficult enough to navigate today with chart and beacons. Moreover, the people were baffled by the different colors of the water. As they came upon the shoals from the deep blue of the gulf, the water at first was clear as crystal, but suddenly turned an opaque green; then after a few miles went milk-white, and finally turned black as ink. And so it is today. Part of the gulf has a bottom of fine white marl which becomes so roiled by the waves that it mixes with the water right up to the surface, looking, as P<sup>er</sup>ter Martyr said, as if flour had been dredged into the sea. I have myself seen the water a deep green, as in the gulf of Maine, although the depth was less than three fathoms, and the next time I looked over the side it was black as ink under a bright sky, owing I suppose to fine black sand on the bottom being stirred up by the waves. All this was new to the Spaniards, and the more terrible because it recalled old Arabic tales of the Green Sea of Gloom, and interminable shoals that fringed the world's outermost edge.<sup>5</sup>

This is exactly where we ran aground in 1948, sailing on a neap tide with centerboard lifted. Within two hours of entering the archipelago, over speckled waters, we ran the ketch onto a shoal and found ourselves dug into the marl just as the unusually high tide began to recede.

It took us days to get off that shoal. Our short-wave radio transmissions could not reach Coast Guard station CLT in Havana because of the intervening Sierra Maestra. We could not find more than a few fathoms of water to float the boat any less than several thousand yards away, which meant kedging off yard by yard. A couple of us in a dinghy would take our heaviest anchor and chain out to a point two or three hundred feet away in the direction of the deeper water, set the anchor solidly in the marl, and then come back so that all hands on the boat could tail the lines through the mechanical winch at the bow to haul the ketch on its side across the sands to deeper water. Kedging is backbreaking work, and because of our pitch on the shoal, we weren't able to draw much drinking water out of the tanks and had to open canned vegetables to get enough liquid to survive. The midsummer sun was unforgiving. Finally, on the fourth day, we pulled free from the suction of that terrible place. Once liberated, we abandoned our plans for continuing to shadow Columbus in his fruitless search for the Chinese mainland and headed for blue water. Refueling in the penal Isle of Pines, *Chalene* continued west, plowing into the Yucatán Channel in time to catch the summer offshore winds and sail east by northeast back to Havana and our mooring in the Rio Almendares.

There were a number of large yachts moored in the Rio Almendares then, including an immense black schooner belonging to a North American sugar plantation owner who, after the revolution, was accused by the Castro government of being a CIA spy. To my boy's eyes, the most intriguing vessel was a former U.S. Navy PT boat, moored right across from us near the other bank of the river where the old colonial government had turned one of its jails into a quarantine confinement. With its prodigious Packard engines rumbling, the PT

<sup>5</sup> Morison, *Admiral of the Ocean Sea*, 460.

boat came and went at the oddest times, slipping its mooring in the early morning hours and returning late the next night. Owner and crew kept strictly to themselves. The sailmaker told me that they were smugglers who charged illegal Chinese immigrants vast sums of money to put them ashore in the Florida Keys. Rumor also had it that, as often as not, the PT boat captain collected the usual U.S. government bounty reward by telling Immigration Service agents just where to wait when he landed the Chinese. And if perchance they were chased by the U.S. Coast Guard, the cold-blooded smugglers deep-sixed their hapless human cargo in the Gulf Stream, taking their lives as casually as they had their money.

All this scuttlebutt bolstered my image of the Chinese as passive victims, meek as lambs led to slaughter. Like the Arawaks chewed up by the dog of Columbus, they seemed just one more pathetic example of the victimization wreaked upon non-Europeans by their Western conquerors. The boldness and daring I so boyishly admired in Columbus was seemingly only the nobler side of an unredeemable history of base domination, brutal extraction, and cruel enslavement. Who illustrated this better in colonial Cuba than the slaves brought in from Africa or the indentured workers imported from China to work the sugar plantations of their Hispanic masters?<sup>6</sup>

The Cuban *hacendados* turned to the importation of indentured Chinese plantation workers both because of labor shortages that resulted from British enforcement of the abolition of the slave trade and because of fears that the African slaves already in Cuba might revolt as Toussaint l'Ouverture's followers had in Haiti.<sup>7</sup> Early in 1846, after a black slave uprising two years before, the Comisión de Población Blanca de la Junta de Fomento approved a plan to introduce Chinese contract labor.<sup>8</sup> Hence, on June 3, 1847, there arrived in Havana aboard the Spanish brig *Oquendo* some 206 indentured laborers from Fujian (the Mangi of Marco Polo and Columbus): the first group of Chinese to land on Cuban soil.<sup>9</sup>

Before abolition, the major promoters of the Chinese coolie trade had transported slaves from Africa.<sup>10</sup> They negotiated their initial coolie contracts through Manila merchants with commercial links to the Amoy agency houses of Tait and Company. Mr. Tait, who was to become the largest shipper of coolies in Amoy, was also consul for Spain, Holland, and Portugal, and he was thus able personally

<sup>6</sup> Rebecca J. Scott, *Slave Emancipation in Cuba: The Transition to Free Labor, 1860-1899* (Princeton, N.J., 1985), 29.

<sup>7</sup> It has also been suggested that the introduction of steam-driven equipment into the sugar refineries required a more skilled work force than African slaves. Denise Helly, "L'émigration chinoise à Cuba," in *Chinois d'outre-mer*, Proceedings of the 29th International Congress of Orientalists, Paris, July, 1973 (Paris, 1976), 61-62. Plans were made to attract white agricultural laborers from Catalan, the Canary Islands, and Galicia by offering high wages, but probably because of the severe working conditions on the plantations, few actually came. Duvon Clough Corbitt, *A Study of the Chinese in Cuba, 1847-1947* (Wilmore, Ky., 1971), 2-3. See also Seymour Drescher, "British Way, French Way: Opinion Building and Revolution in the Second French Slave Emancipation," *AHR*, 96 (June 1991): 710-11.

<sup>8</sup> The Junta was a government-sponsored corporation of prominent planters and businessmen, first organized in 1795.

<sup>9</sup> Juan Pérez de la Riva, *Para la historia de las gentes sin historia* (Barcelona, 1976), 47-65.

<sup>10</sup> Pedro Zulueta, the first importer of Chinese laborers, had been tried in London for violating the 1817 and 1835 treaties between England and Spain abolishing the slave trade; Corbitt, *Study of the Chinese in Cuba*, 4-5.



to certify the legality of his own indenture contracts.<sup>11</sup> As the trade flourished, the agency houses began to bypass the Manila middlemen by turning to Liverpool, Boston, and New York shippers, on the one hand, and, on the other, by dealing directly with the Cuban importers through Macao, where letters of credit from Havana, drawn on London or Paris, were exchanged in Hong Kong banks for Mexican silver dollars to pay the individual brokers a commission of 5 to 10 pesos for each coolie who was contracted. In this fashion, the Catalan dealer, Abellá Raldiris, alone “embarked” more than 100,000 Chinese for Havana, Callao, California, Australia, and Arkansas.<sup>12</sup>

The brokers, or crimps, in Macao, Amoy, Swatow, Hong Kong, and Whampoa who engaged Chinese to be carried to Cuba were often “chinos ladinos” of Sino-Portuguese descent, who would entice their victims into a teahouse, promise that they would be taken to Tay Loy Sun (Da Lüsung, Luzon) or “Great Spain” to make their fortune, pay them 8 silver dollars to sign an eight-year indenture agreement, and then decoy them to the depositories or barracoons, which the Chinese called *zhuzi guan*, or “pig pens.”<sup>13</sup> The conditions inside these filthy enclosures, where no small number of these emigrants succumbed to disease, were inhuman.<sup>14</sup> The Chinese, thereafter to be called “coolies,” were stripped of their clothing, disciplined with salted cat-o’-nine-tails, and penned to await the next clipper ship sailing for the sugar plantations of Cuba or the guano mounds of the Chincha Islands, where they frequently died under the whips of their Peruvian overseers or suffocated in clouds of guano dust.<sup>15</sup>

Surviving the voyage itself was an ordeal. “We proceeded to sea, we were confined in the hold below; some were even shut up in bamboo cages, or chained to iron posts, and a few were indiscriminately selected and flogged as a means of intimidating all others; whilst we cannot estimate the deaths that, in all, took place, from sickness, blows, hunger, thirst, or from suicide by leaping into the sea.”<sup>16</sup> The American clippers in the coolie trade had more space below deck than the British guineamen in the slave trade, where, during the horrible “middle voyage” between Africa and the Americas, “each living man had less room than a dead man in his coffin.”<sup>17</sup> But maltreatment and disease took their toll in the coolie trade as well.<sup>18</sup> According to Cuban census figures, from 1848 to 1874, 141,391

<sup>11</sup> Robert L. Irick, *Ch'ing Policy toward the Coolie Trade 1847–1878* (Taipei, 1982), 27.

<sup>12</sup> Juan Pérez de la Riva, *El barracón: Esclavitud y capitalismo en Cuba* (Barcelona, 1978), 89–92, 101.

<sup>13</sup> Juan Jiménez Pastrana, *Los chinos en la historia de Cuba, 1847–1930* (Havana, 1983), 31–32.

<sup>14</sup> “After entering, the gates were closed by a foreigner, and as all exit was prevented we perceived how we had been betrayed, but there was no remedy; in the same chambers were more than 100 others, most of whom passed their days and nights in tears, whilst some were dripping with blood—the result of chastisements inflicted on account of a suspected intention of escape, or of a declaration of their unwillingness, when interrogated by the Portuguese inspector. The barracoon was of great depth, and, at the time of punishment, as an additional precaution to prevent the cries being overheard, gongs were beaten, and fireworks discharged, so that death even might have ensued without detection”; deposition of Ye Fujun in *Report of the Commission Sent by China to Ascertain the Condition of Chinese Coolies in Cuba* (Taipei, 1970), 9.

<sup>15</sup> Basil Lubbock, *Coolie Ships and Oil Sailors* (Glasgow, 1981), 32–35; Irick, *Ch'ing Policy*, 27.

<sup>16</sup> Deposition of Li Zhaochun in *Report of the Commission Sent by China*, 12.

<sup>17</sup> Lubbock, *Coolie Ships*, 11.

<sup>18</sup> “On board 300 died from thirst”; deposition of Chen Asheng in *Report of the Commission Sent by China*, 13. “Eleven men committed suicide. The day after I embarked we were all ordered on deck, and foot irons were attached to 173 physically strong men, besides 160 men were stripped and

Chinese were shipped to Havana; 16,576 died en route; and 124,813 were "sold" in Cuba.<sup>19</sup> One of the major causes of death en route was cholera; and if a ship so afflicted sailed into Havana harbor, its cargo was quarantined for forty days in the "lazareto de la Chorrera" at the mouth of the Almendares River, where the *Chalene's* mooring was set when I was a boy.<sup>20</sup>

Once off the ship in Havana, the Chinese laborers were "offered for sale in the men-market," where they were forced, to their great shame, to strip naked and be prodded and poked like horses by the buyers.<sup>21</sup> After being sold, the Chinese laborers were taken to sugar plantations, confined in barracks, and sent to work in the fields and mills under armed overseers.<sup>22</sup> Field hands were cowed by sword-bearing "captains," whose soldiers cut off the laborers' queues. According to the testimony of a Chinese plantation worker: "We are fed worse than dogs, and are called upon to perform labour for which an ox or a horse would not possess sufficient strength. Everywhere cells exist, and whips and rods are in constant use, and maimed and lacerated limbs are daily to be seen."<sup>23</sup> Millworkers were paid much lower wages than free workers or rented slaves, frequently whipped and chained in spite of the abolition of corporal punishment in 1854 and often forced to sign fresh contracts of indenture when their eight-year terms concluded. In short, the Chinese quickly came to see that they were debt peons being treated, in Rebecca Scott's words, "as slaves by an incomparably barbarous group of foreigners who refused to recognize them as free men."<sup>24</sup>

One alternative to this misery was death. "Suicides by hanging on trees, by drowning, by swallowing opium, and by leaping into the sugar caldrons are the results of wrongs and sufferings which cannot be described."<sup>25</sup> During the 1860s, the rate of suicide for Chinese in Cuba was 500 in 100,000, compared to 35 in 100,000 for slaves and 5.7 in 100,000 for whites. That is to say, Chinese committed suicide one hundred times more than whites and fourteen times more than slaves. As a result, Cuba had the highest suicide rate in the world: 1 in 4,000 inhabitants.<sup>26</sup>

Another alternative was to resist, to fight back, and the Chinese coolies, far from being passive, did just that. From the moment they entered the barracoon, they tried to escape—sometimes by going through openings in the water closet into the mud and filth of the river.<sup>27</sup> The coolie clippers had to be built like the old convict ships, with gratings of strong iron bars bolted onto each hatchway. Not only that;

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flogged on their naked persons with rattan rods"; deposition of Huang Afang in *Report of the Commission Sent by China*, 15.

<sup>19</sup> Virtually all were men. Only 20 to 30 women per year came to Cuba. Of course, the direct coolie trade was not the only source of Chinese immigrants to Cuba. After 1860, as many as 25,000 Chinese came to Cuba from California via Mexico and New Orleans. Pérez de la Riva, *El barracón*, 56–58.

<sup>20</sup> Pérez de la Riva, *El barracón*, 107.

<sup>21</sup> Deposition of Li Zhaochun in *Report of the Commission Sent by China*, 18.

<sup>22</sup> The auction was technically a sale of their contracts of indenture.

<sup>23</sup> Petition of Xian Zuobang in *Report of the Commission Sent by China*, 19.

<sup>24</sup> Scott, *Slave Emancipation in Cuba*, 33. See also the petitions and depositions in *Report of the Commission Sent by China*, 23.

<sup>25</sup> Petition of Yang Yun in *Report of the Commission Sent by China*, 20.

<sup>26</sup> Pérez de la Riva, *El barracón*, 67.

<sup>27</sup> Irick, *Ch'ing Policy*, 27.

many clippers had barricades ten feet high in front of the poop and were manned by armed sentries to keep the Chinese from breaking out of the hold and storming the helm of the ship. But break out they did.<sup>28</sup>

One of the most famous mutinies took place in 1859 aboard the *Norway*, an unusually large ship registered in New York, that was carrying a thousand Chinese laborers from Macao to Havana. The fifth night out of Macao, fire erupted in the hold, and the Chinese fought ferociously to get to the deck. The heavily armed crew barely held them off. At one point, the mutineers sent the captain a message written in the blood of their wounded, demanding that the ship change course for Siam so that those who wanted to leave the vessel could flee ashore. But, in the end, the men failed to break out of the heavily barred hold, and the fire was extinguished. By the time the *Norway* reached Havana, 130 of the Chinese were dead: 70 from wounds, the rest carried away by dysentery.<sup>29</sup>

The Chinese also resisted ashore. In November 1852, demonstrations broke out in Amoy, with the protesters demanding that the "pig trade" cease and that the foreign agency houses and their Chinese brokers be punished. When the British landed a force from H.M.S. *Salamander* to protect their nationals, the Fujianese forced the British to retreat. The British soldiers killed and wounded 10 or 20 Chinese as they fell back, but the ensuing investigation by Her Majesty's government along with continuing protests by the gentry and people of Amoy marked the beginning of the decline of the Amoy coolie trade and the beginning of "a pattern of popular interference with the trade . . . that was to follow the trade wherever it went."<sup>30</sup>

On the other end, in Cuba itself, the Chinese continued to rebel.<sup>31</sup> By 1848, as large numbers of Chinese fled plantations, the Spaniards began to realize that the Chinese might be good workers, but they were not submissive and certainly were not resigned to being governed "a palos." Frequent uprisings by Chinese who had taken to the hills led to the issuance of special regulations in April 1849 for the punishment of Asiatic recalcitrants: floggings, imprisonment, and solitary confinement.<sup>32</sup> Nonetheless, in August 1860, the captain general, Francisco Serrano, wrote to Madrid urging that the government "put a stop to the damages caused in Cuba by the entry of Chinese who failed to live up to their contracts, broke the laws of hospitality, disturbed public order, aided the enemies of the nation, and kept the Island in a constant state of alarm."<sup>33</sup>

During the 1868–1869 insurrection in Cuba, the insurgents offered liberty to any slaves and coolies who would join them. Especially in the central provinces, many Chinese joined the rebel ranks, including former Taiping Heavenly Kingdom followers who participated decisively in the assault on Manzanillo.<sup>34</sup>

<sup>28</sup> Basil Lubbock, *The China Clippers* (1914; Taipei, 1966), 44–49. See, for example, the account of the successful mutiny of the Chinese aboard the *Robert Browne* in 1852, recounted in Irick, *Ch'ing Policy*, 32–43.

<sup>29</sup> Lubbock, *Coolie Ships*, 43–48.

<sup>30</sup> Irick, *Ch'ing Policy*, 32.

<sup>31</sup> Scott, *Slave Emancipation in Cuba*, 33–34.

<sup>32</sup> Jiménez Pastrana, *Los chinos en la historia de Cuba*, 47–48.

<sup>33</sup> Corbitt, *Study of the Chinese in Cuba*, 21–22.

<sup>34</sup> Juan Jiménez Pastrana, *Los chinos en las luchas por la liberación cubana, 1847–1930* (Havana, 1963), 71–79; Scott, *Slave Emancipation in Cuba*, 57–58.

This restiveness coincided with growing international indignation over the coolie trade to Cuba and the forced retirement of American vessels from the transport during our Civil War. The Qing government, after considerable Spanish stonewalling, managed to send a delegation from its newly formed Zongli Yamen (Foreign Office) in 1873 to investigate the condition of Chinese workers in Cuba.<sup>35</sup> The delegation's report was a devastating exposé of the *hacendados'* callous exploitation of Chinese laborers; and, in due course, on November 17, 1877, the Spanish envoy in Beijing signed a treaty permanently closing the coolie trade with Cuba.<sup>36</sup>

Contrary, then, to my boyhood image of passive coolies meekly victimized by their exploiters, the Chinese laborers' historical experience in Cuba from 1846 to 1877 reflected much active resistance—defiance to the point of forcing the colonial government to stop the trade. But was that so surprising, given the nature of the men themselves? After all, many of these Cantonese and Fujianese laborers were decoyed into the barracoons in the first place because they were willing to set sail, so to speak, without a sure guarantee of return. Their maritime provinces were lands with a long tradition of deep sea navigation that surely made the prospect of an odyssey to "Great Spain" less terrifying than such a voyage would have seemed to a landlocked native of Henan or Shanxi. These Chinese of the southeastern coast were, after all, heirs to the naval tradition of China's greatest explorer, Zheng He.

I FIRST LEARNED OF THE SEA VOYAGES OF ZHENG HE, the Chinese admiral who sailed to the coast of Africa and back in the early fifteenth century, when I was a beginning graduate student at Berkeley. My professor, the late Joseph Levenson, used the example of Zheng He's voyages not so much to illustrate China's awesome technological achievements but to note how the termination of the voyages in 1433 marked a cultural *volte-face* as Ming China turned back on itself and rejected the outside world. I certainly accepted the latter point, but I was most impressed by the revelation that China had once been a great sea power. Professor J. P. Lo at nearby Davis taught me not only that the Song (960–1278) and Yuan (1279–1368) dynasties had deployed large navies in Southeast Asia and against Japan but also that the Ming dynasty (1368–1644) had at least during the first sixty-five years of its existence strongly depended on naval might.<sup>37</sup> Under the

<sup>35</sup> Cuba Commission, *Chinese Emigration: Report of the Commission sent by China to Ascertain the Condition of Chinese Coolies in Cuba* (Shanghai, 1876). This has been reproduced and is cited here as *Report of the Commission Sent by China*.

<sup>36</sup> Corbitt, *Study of the Chinese in Cuba*, 19–20; Jiménez Pastrana, *Los Chinos en las luchas por la liberación cubana*, 88.

<sup>37</sup> The first Chinese admiralty was established by the Southern Song in 1132, and its fleet quickly gained control of the East China Sea. Song and Mongol navies clashed in 1277, and the final decisive conflict between them was the sea battle off the Guangdong coast in 1279, in which the Mongols captured 800 Chinese warships. Khubilai Khan unsuccessfully attempted to invade Japan in 1274 with 900 warships and failed again in 1281 with 4,400 vessels. Joseph Needham, with the collaboration of Wang Ling and Lu Gwei-djen, *Science and Civilisation in China*, Volume 4, *Physics and Physical Technology*, Part 3, *Civil Engineering and Nautics* (Cambridge, 1971), 476–77. A major factor contributing to the Ming founder's rise to power was the naval campaign of 1363 on Lake Poyang, resulting in Zhu Yuanzhang's gaining mastery over the Yangzi Valley. Edward L. Dreyer, "The

Yongle emperor (r. 1403–1424), the Ming navy consisted of 3,500 ships, which conducted annual armadas well off the coast, pursued Japanese “sea rovers” (*wokou*) as far as the Ryukyus and the shores of Korea, helped the Chams drive off an Annamese fleet in 1403, and invaded the Red River delta in 1407 to reannex that part of Annam as a Chinese province.<sup>38</sup>

In 1405, the Yongle emperor—who had usurped the throne of China from his nephew, the Jianwen emperor (r. 1399–1402)—ordered his chief eunuch, Zheng He, to conduct a massive naval expedition beyond Annam and through the Straits of Malacca into the “western seas” (*xiyang*).<sup>39</sup> The ostensible reason for the expedition was to pursue the Jianwen emperor across Southeast Asia.<sup>40</sup> But the real purposes of the voyage were, first, to impress China’s neighbors with the prosperity and power of the new dynasty, which had driven the Mongols beyond the Great Wall; second, to gain access to luxury products no longer available because the breakup of the Mongol empire had severed trade routes; and, third, to encourage embassies to come and pay tribute to the court of the new Yongle emperor.<sup>41</sup> A eunuch was chosen to lead the expedition because, ever since the Han dynasty (206 B.C.–220 A.D.), eunuchs were responsible for purveying articles of luxury for the court, including the emperor’s harem.<sup>42</sup> And among the

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Poyang Campaign, 1363: Inland Naval Warfare in the Founding of the Ming Dynasty,” in Frank A. Kierman, Jr., and John K. Fairbank, eds., *Chinese Ways in Warfare* (Cambridge, Mass., 1974), 202–03.

<sup>38</sup> Jung-pang Lo, “The Decline of the Early Ming Navy,” *Oriens extremus*, 5 (1958): 150–51.

<sup>39</sup> This could also be translated as “western route,” since it was the term employed by Chinese navigators for the passage across the “south seas” (*nanhai*) all the way to Africa. Yün-ts’iao Hsü, “Notes on Some Doubtful Problems Relating to Admiral Cheng Ho’s Expeditions,” in *Chinois d’outre-mer*, 74–75. That would accord nicely with the Chinese charts of the passage depicting “a schematic corridor in which sailing tracks are marked with precise compass-bearings and other instructions.” Joseph Needham and Wang Ling, *Science and Civilisation in China*, Volume 3, *Mathematics and the Sciences of the Heavens and the Earth* (Cambridge, 1959), 560.

<sup>40</sup> When the Yongle emperor—then still the prince of Yan—took Nanjing in July 1402, the bodies of the empress and her eldest son were found within the burned inner palace. There were rumors that the Jianwen emperor had escaped, although the new government announced that his remains had been found and would be buried with the other two corpses. The rumors persisted and were perpetuated by historians such as Gu Yingtai (d. ca. 1689), who claimed that the Jianwen emperor had escaped to southwestern China and lived until 1440. Gu Yingtai, *Ming shi jishi benmo* [Narratives of Ming history from beginning to end] (Taipei, 1976), 198–206. For a contemporary recount of this version, see Shang Chuan, *Yongle huangdi* [The Yongle emperor] (Beijing, 1989), 131–39. Most modern historians believe that the Jianwen emperor died in the palace blaze; see Edward L. Dreyer, *Early Ming China: A Political History, 1355–1435* (Stanford, Calif., 1982), 169; Harold L. Kahn, *Monarchy in the Emperor’s Eyes: Image and Reality in the Ch’ien-lung Reign* (Cambridge, Mass., 1971), 12–37.

<sup>41</sup> Some historians have claimed that the first Zheng He expedition was part of the Yongle emperor’s plan to acquire allies in the Western Oceans and attack Temür [Tamerlane] (1335–1405) on his flank through India. Temür had been planning to invade the Ming since 1398, and in December 1404 he left Herat at the head of some 200,000 warriors. Chung-jen Su, “Places in South-east Asia, the Middle East and Africa Visited by Cheng Ho and His Companions (a.d. 1405–1433),” in F. S. Drake, ed., *Symposium on Historical, Archaeological and Linguistic Studies on Southern China, South-east Asia and the Hong Kong Region* (Hong Kong, 1967), 198. However, Rossabi authoritatively concludes that there was no connection between the launching of the Zheng He expeditions and Temür, who died en route to China on February 18, 1405. His son and successor, Shâhrukh Bahâdur, made an accommodation with the Ming court. Morris Rossabi, “Cheng Ho and Timur: Any Relation?” *Oriens extremus*, 20 (1973): 134–35. See also Joseph F. Fletcher, “China and Central Asia, 1368–1884,” in John K. Fairbank, ed., *The Chinese World Order: Traditional China’s Foreign Relations* (Cambridge, Mass., 1968), 209–11.

<sup>42</sup> J. J. L. Duyvendak, *China’s Discovery of Africa* (London, 1949), 26–27; Hsü, “Notes on Some



emperor's most trusted eunuchs, Zheng He may have been especially well qualified because he was a Yunnanese Muslim (both his father and grandfather were hajis who had made the pilgrimage to Mecca) and because he was an excellent military commander and logistician who had played a key role in the Yongle emperor's victorious military campaigns.<sup>43</sup>

The armada was immense, especially when we compare it to the *Santa Maria*, *Niña*, and *Pinta* that set off from the Canary Islands eighty-seven years later.<sup>44</sup> Altogether, there were 62 huge nine-masted galleons called "treasure junks" (*baochuan*), 450 feet long and 180 feet across the beam. Since the upper decks and poops of the galleons overrode the bottom, the waterline length and beam were probably closer to 310 and 80 feet. However, a vessel that large would have displaced at least 3,000 tons, whereas none of Vasco da Gama's ships exceeded 300 tons, and even in 1588 the largest English merchant ship did not exceed 400 tons.<sup>45</sup> The nine masts of the *baochuan*, which were built in the Longjiang shipyards on the northwest side of Nanjing, had fore-and-aft sails; the galleons were steered with axially mounted rudders and fitted with strong bulkhead-built hulls divided into watertight compartments kept dry with pedal-driven bilge pumps.<sup>46</sup> The rest of the fleet of several hundred ships consisted of eight-masted

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Doubtful Problems," 73. At this time, China exported silks, porcelains, lacquer ware, art objects, copper cash, iron pans, and Buddhist sutras. It imported camphor, tortoiseshell, coral, pepper, and other spices, areca nuts, sandalwood, incense, dye stuffs, cotton fabrics, sugar, ivory, elephants, parakeets, buffaloes, pearls and precious stones, rhinoceros horns, drugs, glass, and tin. It also imported horses, copper ore, sulphur, timber, hides, gold, silver, and rice. Ma Huan, *Ying-yai Sheng-lan: The Overall Survey of the Ocean's Shores* [1433], translated from the Chinese text edited by Feng Ch'eng-chün, with introduction, notes, and appendices by J. V. G. Mills (Cambridge, 1970), 4.

<sup>43</sup> Ma Jizu and Zheng Yunliang, "Weida de hanghaijia Zheng He ji qi jiaoshi" [The great navigator Zheng He and the state of his family], in Yunnan Provincial Editorial Group, ed., *Yunnan huizu shehui lishi diaocha* [Investigations into the social history of Yunnan Muslims] (Kunming, 1987), no. 4: 43–44; Chung-jen Su, "Places in South-east Asia," 198. Zheng He's original name was Ma He; he was also colloquially called Ma Sanbao. The "san bao," or three jewels, represented the Buddhist *triratna* (Buddha, Dharma, Sangha). One later text suggests that the "san bao" also referred to the "three precious eunuchs" appointed by Yongle to head the expedition: Zheng He, Yang Min, and Li Kai; Hsü, "Notes on Some Doubtful Problems," 71–72. For distinguished military service, the emperor conferred the surname of Zheng on Ma He in 1404 and promoted him to be superintendent of the office of eunuchs; Ma Huan, *Ying-yai Sheng-lan*, 5–6.

<sup>44</sup> It even dwarfed the Spanish armada, which consisted of 28 galleons, 40 large armed merchantmen, 34 fast ships, 23 freighters, and 4 Portuguese galleys, and which carried about 10,000 soldiers.

<sup>45</sup> Ma Huan, *Ying-yai Sheng-lan*, 31. Needham takes J. P. Lo's calculations of *liao* (see below, n. 46) to estimate a burden of 500 tons. Needham, *Science and Civilisation in China*, 4: 480–81.

<sup>46</sup> Nathan Sivin, "Review of *Science and Civilisation in China, Volume 4: Physics and Physical Technology; Part III: Civil Engineering and Nautics*, by Joseph Needham, with the collaboration of Wang Ling and Lu Gwei-Djen," *Scientific American* (January 1972), 113. See also Paul Pelliot, "Les grands voyages maritimes chinois au début du XV<sup>e</sup> siècle," *T'oung Pao*, 30 (1933): 273–74. Historians have been reluctant to accept the "monstrous" sizes given for the *baochuan* in the *Ming shi*. However, over the course of the seven Zheng He expeditions, the average size of the complement of a single vessel was 500 men, which would have required a ship of at least 2,000 *liao* (a unit of ship measurement that came to about 500 lbs.). Ships of this size were mentioned by Marco Polo and Ibn Batuta. In a stele discovered in 1936 at the Jinghai temple near Nanjing, there is a discernible portion of the text that speaks of the 1405 command having 2,000-*liao* seagoing ships, and in the 1409 command of 1,500-*liao* seagoing ships. Jung-pang Lo, "The Decline of the Early Ming Navy," 151. In 1962, a rudder post over 36 feet long and 1.25 feet in diameter with a rudder attachment length of nearly 20 feet was discovered in the ruins of the old Ming shipyard in Nanjing. Such a rudder would have



"gallopers" (*machuan*), seven-masted grain junks (*liangchuan*), six-masted transports (*huochuan*) and five-masted combat vessels (*zhanchuan*).<sup>47</sup>

When the fleet was assembled near present-day Shanghai, it carried 17 imperial eunuch ambassadors and assistant ambassadors; 63 eunuch officials and chamberlains; 95 military directors; 207 brigade and company commanders; 3 senior ministry secretaries; 2 masters of ceremony from the department of state ceremonials; 5 geomancers; 128 medical personnel; and 26,803 officers, soldiers, cooks, purveyors, clerks, and interpreters.<sup>48</sup>

From the Yangzi River, Zheng He's fleet sailed down the coast to Fujian—Marco Polo and Columbus's "Mangi"—and anchored in the Min River estuary. When the northeast monsoon began to blow in December and January, Zheng He made offerings to Tianfei, the "Celestial Spouse" who protects mariners and is today worshiped as the goddess Mazu throughout coastal Fujian and Taiwan, and then he set sail for Champa (Indochina).<sup>49</sup> From there, the armada advanced to Java, Sumatra, Ceylon, and Calicut on the west coast of India. By the time Zheng He was ready to return to China in April 1407, his suite contained envoys from nearly all of those tributaries along with the truculent Palembang sealord Chen Zuyi, who was brought back to Nanjing to be decapitated.<sup>50</sup>

There were six more of these impressive voyages, progressively extending farther westward. Zheng He was not on every one of them, but he commanded them all. The second expedition (1407–1409) was launched to install the new king of Calicut, Mana Vikraman. During the third expedition (1409–1411), on his way back to China, Zheng He was attacked by the king of Ceylon (probably Bhuvaneka Bâhu V). Zheng He defeated the Sinhalese army and captured the royal family, which was taken back to Nanjing and presented to the emperor.<sup>51</sup> Yongle freed the king and his family and sent them back to Ceylon.<sup>52</sup> That action, along with the establishment of Chinese commanderies in Tonkin and Upper Annam, greatly increased the number of tributaries coming to the Ming court.<sup>53</sup>

The fourth expedition (1413–1415) followed the same initial route as the earlier ones but this time sailed even farther, visiting the Maldiv Islands, reaching the Persian sultanate of Ormuz, and sending a branch expedition to Bengal that brought back to China envoys from the African kingdom of Malindi, who presented the Yongle emperor with a giraffe.<sup>54</sup> This was an extremely

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had a surface area of 452 square feet, proving that such immense vessels did indeed exist. Needham, *Science and Civilisation in China*, 4: 481.

<sup>47</sup> Su Chung-jen, "Places in South-east Asia," 200–01.

<sup>48</sup> Paul Pelliot, "Les grands voyages maritimes chinois," 273–74; Su Chung-jen, "Places in South-east Asia," 201.

<sup>49</sup> J. J. L. Duyvendak, "The True Dates of the Chinese Maritime Expeditions of the Early Fifteenth Century," *T'oung Pao*, 34 (1938): 342–44; Zhongguo hanghai shi yanjiu hui [Society for the study of Chinese maritime history], eds., *Guangdong haiyun shi (gudai bufen)* [History of Chinese maritime transport (Ancient part)] (Beijing, 1989), 159–61.

<sup>50</sup> Ma Huan, *Ying-yai Sheng-lan*, 10–11; Pelliot, "Les grands voyages maritimes chinois," 273–77.

<sup>51</sup> Needham, *Science and Civilisation in China*, 4: 516.

<sup>52</sup> However, the Chinese insisted that the king be replaced as ruler by his cousin. Dreyer, *Early Ming China*, 197.

<sup>53</sup> *Ming shi* [History of the Ming], 6:3b, transl. in Pelliot, "Les grands voyages maritimes chinois," 279–80. See also Ma Huan, *Ying-yai Sheng-lan*, 11–12.

<sup>54</sup> Ma Huan, *Ying-yai Sheng-lan*, 12–13.

auspicious gift because the giraffe—whose name in Somalian is *girin*—was taken to be the *qilin* or unicorn, the appearance of which was the sign of a sage-emperor whose presence attracted “distant people . . . in uninterrupted succession.”<sup>55</sup> In return for this homage, Yongle sent Zheng He on a fifth voyage (1417–1419) to accompany the Malindian ambassadors home. This was probably the first time that Zheng He reached the east coast of Africa. He made a display of military force at Mogadishu in Somaliland, while ships detached from the main fleet sailed north to the Arabian peninsula. The sixth expedition (1421–1422), which consisted of 41 ships, also reached Africa, going as far as Mogadishu and Brava.<sup>56</sup>

This marked the apex of Ming maritime power. When the Yongle emperor died in 1424, the suzerainty of China was acknowledged by more foreign rulers than ever before; and representatives of 67 overseas states, including 7 kings, came bearing tribute.<sup>57</sup> Yet hardly was Yongle laid away than his short-lived successor, the Hongxi emperor (r. 1425), halted the expeditions and appointed Zheng He the defender of Nanjing. There was a final seventh expedition in 1431, when the Xuande emperor (r. 1426–1435) charged Zheng He with the command of an expedition of 100 vessels that sailed to Ormuz and sent subsidiary fleets to the east coast of Africa and to Mecca in the north.<sup>58</sup> But, after Zheng He returned to Nanjing and resumed his position as defender of the capital in 1433, the voyages ended altogether.<sup>59</sup>

Why did the argosies cease? The most commonly accepted explanation has been that the voyages were compromised from the start by their connection with palace eunuchs, who were associated with extravagance and imperial caprice.<sup>60</sup> As the Dutch Sinologist J.J.L. Duyvendak put it, “The entire business of relations with overseas barbarians became, in the moral and political judgment of the official classes, inextricably bound up with their deep sense of disapproval of the extravagances and usurpation of power of the despised eunuchs.”<sup>61</sup> The shift in policy was so extreme that in 1477 when the eunuch Wang Zhi called for the charts of Zheng He’s voyages in order to make plans to restore China’s paramount position in Southeast Asia, the vice-president of the ministry of war had all the government’s records of the expeditions taken out and burned.<sup>62</sup>

The decline of the Ming navy was precipitate. Far-flung coastal patrols against

<sup>55</sup> Duyvendak, *China’s Discovery of Africa*, 33.

<sup>56</sup> Ma Huan, *Ying-yai Sheng-lan*, 13–14; Needham, *Science and Civilisation in China*, 4: 489–90.

<sup>57</sup> Ma Huan, *Ying-yai Sheng-lan*, 2. See also Wang Gungwu, “Early Ming Relations with Southeast Asia: A Background Essay,” in Fairbank, *Chinese World Order*, 53–54.

<sup>58</sup> Ma Huan, *Ying-yai Sheng-lan*, 14–18; Sivin, “Review of *Science and Civilisation in China*, Volume 4,” 113; Needham, *Science and Civilisation in China*, 4: 490.

<sup>59</sup> Zheng He died not long after in 1435. Ma Huan, *Ying-yai Sheng-lan*, 6.

<sup>60</sup> Needham, *Science and Civilisation in China*, 4: 524–25; Shang Chuan, *Yongle huangdi*, 260–62. “Trade, which Confucianists affected to scorn (while Buddhism gave it impetus), was a matter of imperial interest. It was an interest deriving from a court society’s demands for luxury, which were not approved by Confucianists, and it was manifest in such various phenomena as the eunuch Cheng Ho’s voyages (1403–33), which Confucian historians buried; eunuchs’ prominence, protested by officials in trading-ship control organs; and the Canton system of trade (1759–1839), in which the superintendent, the ‘Hoppo’, was a specifically imperial appointee and outside the regular bureaucratic chain of command”; Joseph R. Levenson, *Confucian China and Its Modern Fate*, Volume 2, *The Problem of Monarchical Decay* (London, 1964), 26–27.

<sup>61</sup> Duyvendak, *China’s Discovery of Africa*, 27.

<sup>62</sup> Needham, *Science and Civilisation in China*, 4: 525.

Sino-Japanese pirates were pulled back after 1436, when the Zhengtong emperor forbade the building of vessels for overseas voyages.<sup>63</sup> The open sea sailors now passively anchored in port engaged in commerce, smuggled salt, or simply deserted their garrisons. The hereditary shipwright households that had built Zheng He's galleons also declined and disappeared, and eventually the Chinese forgot how to construct the giant seagoing vessels of the earlier period.<sup>64</sup>

Many Chinese historians have used the end of Zheng He's voyages to mark the fatal decline of the Ming dynasty. The rise of corrupt palace eunuchs in the 1440s, the neglect of public works after the breach of the Yellow River dikes in 1448, rising taxes with increased court expenses, and the blatant sale of public offices in the 1470s all seemed to signal a decisive dynastic turnabout, although the dynasty had two more centuries of life left.<sup>65</sup>

Even more significant, historians have regarded the termination of the expeditions as a turning point in the history of Chinese civilization itself. Professor Lo took this to be a sea change in the character and temperament of the Chinese, who became more "civilized" and "decadent," preferring "lyrics to techniques, epistemology to politics, and the paintbrush to the sea."<sup>66</sup> Dr. J. V. G. Mills flatly declared, "The passing of the Yongle emperor ended the heroic age of imperial China; the great awakening was over, the spiritual vigor evaporated, and energetic action was no longer forthcoming. Military ardor waned, and anti-militaristic and anti-expansionist sentiments were aired."<sup>67</sup> And Joseph Needham, in a moving if overdrawn comparison of the Portuguese and Chinese maritime efforts, concluded with the observation that "the eunuchs were the architects of an outstanding period of greatness in China's history," and the end of the expeditions indicated that "the great naval possibilities had been done to death."<sup>68</sup>

This was the *repliement*, China's turning back upon itself, that Joseph Levenson had conveyed to me as a graduate student and that permitted me to think of an insular continental empire, closed to the outside world until "strangers at the gate" forced open the barriers in the 1840s and brought China into world history.<sup>69</sup> Of course, one can try to see this supposed introversion in a good light. Instead of the aggressive thrust outward that enriched, engrossed, and then eventually expended the Iberian empires, for instance, China's self-enclosure

<sup>63</sup> In 1411, Chinese engineers constructed dams that converted the Grand Canal into an all-seasons conduit, making it possible four years later for the government to abolish the maritime grain-transport service and thereafter send all tribute grain north to the capital by inland waterway. Sea transport was revived in 1572, but only temporarily. By 1575, the seagoing ships were put in reserve. Hoshi Ayao, *The Ming Tribute Grain System*, Mark Elvin, transl. (Ann Arbor, Mich., 1969), 76-77; Needham, *Science and Civilisation in China*, 4: 315, 526; Wu Jihua, *Mingdai haiyun ji yunhe de yanjiu* [A study of sea transport and canal transport during the Ming period] (Taipei, 1961), 268-74.

<sup>64</sup> Jung-pang Lo, "Decline of the Early Ming Navy," 156-62.

<sup>65</sup> Jung-pang Lo, "Decline of the Early Ming Navy," 164-65.

<sup>66</sup> Jung-pang, Lo, "Decline of the Early Ming Navy," 168. See also John E. Wills, Jr., *Embassies and Illusions: Dutch and Portuguese Envoys to K'ang-hsi, 1666-1687* (Cambridge, Mass., 1984), 17.

<sup>67</sup> Ma Huan, *Ying-yai Sheng-lan*, 3.

<sup>68</sup> Needham, *Science and Civilisation in China*, 4: 525, 527. Sivin concurred: "Cheng's argosies, however, were a final blaze of splendor before the extinction of the large and intrepid navy that had been founded 300 years earlier. The political decisions that killed it were part of a decisive turning inward of the civilization"; Sivin, "Review of *Science and Civilisation in China*," 113.

<sup>69</sup> Frederic Wakeman, Jr., *Strangers at the Gate: Social Disorder in South China 1839-1861* (Berkeley, Calif., 1966), 6-7.

permitted advanced social and cultural development within a single ecumene, contemplative and sophisticated, unriven by narrow ethnic nationalisms and enduring century after century. The reign of the self-restrained and considerate Hongzhi emperor (r. 1488–1505)—the only monogamous emperor in Chinese history—was characterized by later Ming historians as a golden age of Confucian sagely rule.<sup>70</sup> In 1492, just as Columbus thought he was discovering the material riches of Asia in the Caribbean, the Wu master Shen Zhou painted his famous hanging scroll “Night Vigil.” The inscription on the painting reads:

My outward form is slave to external things, and my mind takes its direction from them. Hearing is obscured by the sounds of bell and drum; seeing is obscured by patterns and beauty. This is why material things benefit people seldom, harm them often. Sometimes it happens, though, as with tonight’s sounds and colors, that while they do not differ from those of other times, yet they strike the ear and eye all at once, lucidly, wonderfully becoming a part of me. That they are bell and drum sounds, patterns and beauty, now cannot help but be an aid to the advancement of my self-cultivation. In this way, things cannot serve to enslave man.<sup>71</sup>

To maintain that revered Confucian realm, the imperial state bureaucracy sought to contain the maritime impulses of the coastal provinces. In 1500, it became a capital offense to build seagoing junks with more than two masts; in 1525, coastal officials were ordered to destroy such vessels altogether; and, by 1551, when Sino-Japanese sea rovers were raiding steadily along the littoral, Chinese who put out to sea, even if just for trade, were punished for treacherous collusion with the enemy.<sup>72</sup>

The continual issuance of these proclamations during the sixteenth century reflected the inability of the imperial Chinese state, which had rejected official maritime expansion, to control private seafaring and maritime trade.<sup>73</sup> During the late 1500s and early 1600s, there was a tremendous expansion of Asian trade, fueled in large part by the vast quantities of silver that were carried by galleons from Acapulco across the Pacific to Manila and from there by Chinese merchant mariners to Fujian and Zhejiang in exchange for silks, porcelains, and other luxury goods.<sup>74</sup>

<sup>70</sup> L. Carrington Goodrich, ed., and Chaoying Fang, assoc. ed., *Dictionary of Ming Biography* (New York, 1976), 378.

<sup>71</sup> Translated in James Cahill, *Parting at the Shore: Chinese Painting of the Early and Middle Ming Dynasty, 1368–1580* (New York, 1978), 90.

<sup>72</sup> Needham, *Science and Civilisation in China*, 4: 527. Needham does, however, note that resistance to Japanese pirates kept the Ming navy strong enough to send squadrons between 1592 and 1598 to fight alongside the Korean admiral Yi Sunsin against the invading Japanese fleets of Hideyoshi, 528.

<sup>73</sup> The powerful families of Fujian and Zhejiang that traded with Sino-Japanese pirates were protected by allies at court. “The naval expeditions of Yung-lo’s time had paved the way for a wave of Chinese migration to Southeast Asia. The heyday of the Arab and Persian merchants had passed, the Portuguese had not yet arrived, and, thus, for a century, the Chinese controlled all the commerce in the waters of the East. Private trade supplanted the official tributary trade which the Cheng Ho expeditions helped to bring about.” These private interests may have earlier frustrated attempts to continue the Zheng He expeditions. Jung-pang Lo, “Decline of the Early Ming Navy,” 156–57.

<sup>74</sup> Juan Gonzales de Mendoza wrote in his *Historia de las Cosas mas notables, Ritos y Costumbres del Gran Reyno de la China, sabidas assi por los libros de los mesmos Chinas, como por relación de religiosos y otras personas que an estado en el dicho Reyno* (Rome, 1585) of Chinese merchant-captains trading overseas under confidential licenses from the Chinese government. Three Chinese merchants had been in Mexico and had gone on to visit Spain. See Needham, *Science and Civilisation in China*, 4: 527.

Between 1573 and 1644, the Chinese economy—ever the sink of precious metals—absorbed 26 million Mexican silver dollars, becoming increasingly monetized and commercialized.<sup>75</sup> These trends abated during the global economic and demographic crises of the mid-1600s, when a new maritime ban was in force between 1659 and 1683.<sup>76</sup> But after the Kangxi emperor's (r. 1662–1722) navy defeated the regime of the seafarer Koxinga (Cheng Chengkong) and his heirs on Taiwan in 1683–1684, the ban was lifted and the inflow of silver resumed.<sup>77</sup> By the late eighteenth century, when Chinese merchant “junk traders” monopolized the exchange of “Straits’ produce” from Southeast Asia, China was closely integrated into the world economy, and fluctuations in the silk and tea trades as well as in domestic grain prices followed the ups and downs of the supply of silver in the New World.<sup>78</sup>

The inability of the Chinese government to control private trade was mirrored in the state's difficulty in preventing Chinese people from migrating abroad.<sup>79</sup> The Chinese diaspora commenced before the great Ming argosies, but it was much stimulated by Zheng He's expeditions.<sup>80</sup> During the later years of the fifteenth century, Chinese began to colonize the Malay Archipelago, Java, Sumatra, Borneo, the Sulu Archipelago, and the Philippines.<sup>81</sup> In the sixteenth century, another stream of Chinese settlers began to arrive in Siam, and by the end of the 1600s there were thousands in the capital of Ayutthaya.<sup>82</sup> The Qing (1644–1912) government continued the Ming policy of forbidding emigration.<sup>83</sup> Article 225 of the Qing code read: “All . . . who remove to foreign islands for the

<sup>75</sup> Man-houng Lin, “From Sweet Potato to Silver: The New World and Eighteenth-Century China as Reflected in Wang Hui-tsu's Passage about the Grain Prices,” in Hans Pohl, ed., *The European Discovery of the World and Its Economic Effects on Pre-industrial Society, 1500–1800* (Stuttgart, 1990), 313.

<sup>76</sup> Frederic Wakeman, Jr., “China and the Seventeenth-Century World Crisis,” *Late Imperial China* (June 1986). But Chinese trade with Southeast Asia certainly continued to flourish, and the Shang family that ruled the feudatory of Guangdong obtained much of its revenue from overseas commerce, including trade in textiles with Japan. Wills, *Embassies and Illusions*, 128–29; Zhongguo hanghai shi yanjiu hui, eds., *Guangdong haiyun shi*, 143–49.

<sup>77</sup> John E. Wills, Jr., *Pepper, Guns and Parleys: The Dutch East India Company and China, 1622 [1662]–1681* (Cambridge, Mass., 1974), 195–97. Restrictive policies were resumed during 1717–1727 when the imperial government prohibited commercial shipping to the Philippines, Java, and most parts of Southeast Asia. Lin, “From Sweet Potato to Silver,” 315–16.

<sup>78</sup> Dian H. Murray, *Pirates of the South China Coast, 1790–1810* (Stanford, Calif., 1987), 10; Lin, “From Sweet Potato to Silver,” 327; Chen Shunsheng, “Qingdai Guangdong de yinyuan liutong” [The circulation of silver dollars in Guangdong during the Qing period], in Ye Xian'en, et al., eds., *Ming-Qing Guangdong shehui jingji yanjiu* [Studies on the society and economy of Guangdong during the Ming and Qing] (Guangzhou, 1987), 206–36.

<sup>79</sup> The best brief discussion in any language of the Chinese diaspora is the chapter of that title in Sucheng Chan, *This Bittersweet Soil: The Chinese in California Agriculture, 1860–1910* (Berkeley, Calif., 1986), 7–31.

<sup>80</sup> When Zheng He got to Palembang, he discovered that most of the people residing there were refugees from Guangzhou, Changzhou, and Quanzhou. Su Chung-jen, “Places in South-east Asia,” 206.

<sup>81</sup> Ta Chen, *Chinese Migrations, with Special Reference to Labor Conditions* (Washington, D.C., 1923), 4.

<sup>82</sup> By the mid-nineteenth century, 15,000 Chinese were migrating to Thailand each year. Richard James Coughlin, “The Chinese in Bangkok: A Study of Cultural Persistence” (Ph.D. dissertation, Yale University, 1953), 14.

<sup>83</sup> When the Ming fell, a number of Chinese fled to Southeast Asia, especially to the lands in Cochinchina controlled by the Nguyen lords, outside of the Trinh lords' kingdom in Tonkin. Special “villages of people continuing to be loyal to the Ming” (*Minh-huong-xa*) were established to house these settlers. Chen Jinghe (Ch'en Ching-ho), *Chengtian Mingxiangshe Chen shi zhengpu* [A brief study of the family register of the Trans, a Ming Refugee family in Minh-huong-xa (Central Vietnam)] (Hong Kong, 1964), 6.



purpose of inhabiting and cultivating the same, shall be punished according to the law against communicating with rebels and enemies and consequently suffer death by being beheaded."<sup>84</sup> Individual emperors issued pardons to overseas merchants who returned home, but not until 1727 was the interdiction removed; by then, hundreds of thousands of Chinese were living abroad. A century later, virtually half the 400,000 residents of Bangkok were Chinese immigrants.<sup>85</sup>

Emigration increased dramatically during the nineteenth century, when the coolie trade flourished.<sup>86</sup> Between 1848 and 1854, during the California gold rush, 700,000 Chinese came to California.<sup>87</sup> By the early 1900s, more than eight million Chinese were settled abroad, and they bore with them an economic and political vitality that helped transform China itself.<sup>88</sup> The Revolution of 1911 that overthrew the Qing dynasty was, of course, led by an overseas Chinese, Sun Yat-sen; and the first United Front between the Nationalists and Communists was largely implemented by a Chinese from San Francisco.<sup>89</sup>

That élan has continued to swell, representing the private and now globally significant complex of individual voyages that is changing the economic face of the world.<sup>90</sup> The tremendous competitive strength of what one sociologist has called "entrepreneurial familism"—a form of private commercial and industrial organization that may have emerged in resistance to the power of the Chinese bureaucratic state—has begun to roll back on China itself.<sup>91</sup> The overseas Chinese, who own liquid assets worth nearly \$3 trillion, are investing billions of dollars in mainland China every year, helping to fuel the expansion of the fastest growing economy in the world.<sup>92</sup> The hundreds of thousands of individual voyages that have taken place since Zheng He launched his expeditions in 1405, well before Columbus thought he had discovered Cathay on the south coast of Cuba, may be reaching a certain kind of harbor at last.

My own first voyage to mainland China was in 1974 as an interpreter for a

<sup>84</sup> Cited in Victor Purcell, *The Chinese in Southeast Asia*, 2d edn. (London, 1965), 26. Both the Ming and Qing did little or nothing to protest against the massacres of Chinese in the Philippines in 1603 and 1639 and in Java in 1740. Edgar Wickberg, *The Chinese in Philippine Life, 1850–1898* (New Haven, Conn., 1965), 10–11.

<sup>85</sup> Purcell, *Chinese in Southeast Asia*, 84–85.

<sup>86</sup> Chen, *Chinese Migrations*, 4.

<sup>87</sup> Of these, 95 percent were males. Many re-migrated. C. Livingston Daley, "The Chinese as Sojourners: A Study in the Sociology of Migration" (Ph.D. dissertation, City University of New York, 1978), 21, 188.

<sup>88</sup> Chen, *Chinese Migrations*, 15.

<sup>89</sup> For the role of the overseas Chinese in the 1911 Revolution, see Huang Zhenwu, *Huaqiao yu Zhongguo geming* [Overseas Chinese and the Chinese revolution] (Taipei, 1963). By 1953, the People's Republic of China estimated that there were 11,743,320 overseas Chinese. Stephen Fitzgerald, *China and the Overseas Chinese: A Study of Peking's Changing Policy, 1949–1970* (Cambridge, 1972), 3. There are now approximately 55 million overseas Chinese, counting Taiwan and Hong Kong.

<sup>90</sup> Chinese constitute 4 percent of Indonesia's population and own 75 percent of the country's assets. Chinese in Thailand, 8–10 percent of the population, own 90 percent of the country's manufacturing and commercial assets and half of the bank capital. Only 1 percent of the population in the Philippines is pure Chinese, but Chinese-owned companies account for 66 percent of the sales of the 67 largest commercial outfits.

<sup>91</sup> Siu-lun Wong, *Emigrant Entrepreneurs: Shanghai Industrialists in Hong Kong* (New York, 1988), 172–73.

<sup>92</sup> The overseas Chinese "gross national product" is estimated as being worth \$450 billion. Their liquid assets are equivalent to all of the bank deposits in Japan. "The Overseas Chinese: A Driving Force," *Economist* (July 18, 1992): 21–24.



delegation of American pharmacologists.<sup>93</sup> It was obvious to me as I left on the trip, in the middle of Watergate and the impending defeat of our forces in Vietnam, that the Nixon Doctrine, sound as it seemed, signaled the end of America's empire in Asia. The war in Indochina had created the first of what were to be huge deficits in the federal budget, and although America's technological supremacy in future brushfire engagements like the Gulf War would be a reassurance of our military expertise to come, favorable economic currents were about to flow in another direction toward the rise of a new Asia and certainly a new China. This was signaled by the enormous economic leaps Japan and the "four dragons" were taking and by the vigor of China's response to challenges over the Spratly and Paracel Islands, where some of the world's largest oil reserves lie.

During that 1974 visit to the People's Republic, I saw for the first time the large, stationary marble barge built on the summer palace's Kunming Lake for the Empress Dowager Cixi on her sixtieth birthday in 1894. The moneys that went into that inert memorial to regal self-esteem were diverted from the Chinese fleet, which was virtually obliterated during the naval battle with Japanese battleships and cruisers off the mouth of the Yalu River on September 17, 1894.<sup>94</sup> I remember shaking my head in remembrance of that awesome misplacement of resources as I walked up to the imperial barge and placidly proceeded to tour the monument in a spirit of what must have been indifferent contempt, hidden even to myself. Coming down the last stairwell on my way off this notorious example of imperial Chinese interiority, I found my way blocked by two athletic young men in naval uniforms. They smiled to my smile and as we each gave way, I noticed the designation on their blouses: "Southeastern Navy of the People's Republic of China," which was the military arm leading the first extension of state power into the waters of Southeast Asia since the Zheng He expeditions.

To be sure, there is only one superpower in Asia now, and even though U.S. forces in Okinawa and South Korea will surely be reduced by our new president, the American strategic presence will likely persist well into the next century. But the fact that China is now building a blue-water navy is not nearly as important as the swell of its economy, not to speak of those of the "newly industrializing countries" and of Japan.

I close with this: a kind of provincial cosmopolitanism. The half-century of purely American hegemony is over. The time is here for us to take seriously the challenges to what was an insular, self-enclosed, and racist cultural ethic and to relish the complex diversity of American society. I am now, by choice and inadvertent shaping, a Californian. And although it may sound strange to you after the Los Angeles riots of last April, my pride in that Californian complexion is for its capacity to encompass the resistance of all our individual cultures to the melting pot and for its commitment to the regeneration of a civil society that will allow each of us to share the journey ahead.

<sup>93</sup> See the introduction to Committee on Scholarly Communication with the People's Republic of China, eds., *Herbal Pharmacology in the People's Republic of China* (Washington, D.C., 1975).

<sup>94</sup> John L. Rawlinson, *China's Struggle for Naval Development, 1839-1895* (Cambridge, Mass., 1967), 140-41, 178-85; Bao Zunpeng, *Zhongguo haijun shi* [History of the Chinese navy] (Taipei, 1951), 209-10.



E. P. Thompson speaking before a London peace rally (1981). Courtesy of END (European Nuclear Disarmament), 11 Goodwin Street, London.

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## E. P. Thompson: The Historian as Activist

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MICHAEL D. BESS

IN THE SPRING OF 1980, E. P. THOMPSON set aside his research as a historian and plunged into a passionate and frantic phase of political activism. Only two years before, he had published *The Poverty of Theory*, a lengthy attack against the French philosopher Louis Althusser, whom he accused of undermining human agency and historical self-determination. Now, with his typical mixture of panache and vitriol, Thompson left the realm of theoretical fine points and took on the military establishments of the two Cold War blocs. Can human beings shape their own history? It was almost as if Thompson were setting out to explore through his own actions the recurring theme that had preoccupied him during three decades of historical writing.

In the years that followed, Thompson became one of the leading figures of a vast popular movement named with the double-edged acronym of END (European Nuclear Disarmament). His mail grew to unmanageable proportions, and he was swamped with requests for public appearances and television interviews. In 1984, he engaged U.S. Secretary of Defense Caspar Weinberger in a blistering debate at Oxford University—an encounter in which both sides claimed a moral victory. He became an impromptu expert on military issues and traded the conceptual tools of cultural and social history for a whole new set of ideas in the fields of military technology and strategic affairs.

Thompson's role as an activist historian had begun much earlier, during the 1950s. His career raises many questions about the dilemmas of political engagement among both historians and intellectuals in general. How does political commitment affect one's judgment as a historian? What should be the underlying aims of historical writing? And conversely, in what ways does being a historian affect one's participation in political life?

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"MY FATHER WAS A VERY TOUGH LIBERAL," Thompson told an interviewer in 1976. "He was a continuous critic of British imperialism, a friend of Nehru's and of other national leaders. So I grew up expecting governments to be mendacious and imperialist, and expecting that one's stance ought to be hostile to government."<sup>1</sup> Born in 1924, Edward Palmer Thompson later described his childhood home as a place to which "some of the most interesting men of the preceding generation were frequent visitors"—from poet Robert Bridges to statesman Lord Lothian, from Tagore to Gandhi and Jawaharlal Nehru (particularly vivid was Thompson's memory of himself standing "in awe before the great Jawaharlal, as he asked me about my batting technique").<sup>2</sup> By the early years of World War II, Thompson and his older brother Frank had not only absorbed the liberal convictions of their parents, they had also gone several steps farther, joining the British Communist Party as soon as they entered university. Thompson, then in his late teens, shared his brother's enthusiasm for a popular and democratic conception of Communism that had not yet been crushed by the experiences of Stalinism. "My brother's surviving letters," he later explained, "are totally at odds with the cardboard ideological picture of what Stalinism was. His commitment was to people and above all to the astonishing heroism of the Partisan movements of southern Europe."<sup>3</sup> As soon as they were old enough to enlist, both brothers joined the fight against fascism. Edward commanded a tank troop in the Sixth Armored Division, fighting in northern Africa and then in Italy; Frank, who had volunteered for a dangerous mission behind Nazi lines into Bulgaria, was captured and executed in 1944, along with a band of local partisans.

In the decade after the war, Thompson immediately began facing the problem of juggling political activism and an academic career. In 1948, he married a fellow Communist, Dorothy Sale, who was pursuing research on Chartism and the nineteenth-century labor movement. During the same year, he secured a job in an adult-education program linked to Leeds University—one of the last of these positions available to Communist intellectuals before a British "mini-McCarthyism" set in.<sup>4</sup> Alongside his teaching, however, Thompson devoted at least half his time to the politics of the Left. He was simultaneously chair of the Halifax Peace Committee, secretary of the broader Yorkshire Federation of Peace Organizations, editor of a peace journal, and a member of the Yorkshire District Committee of the Communist Party.<sup>5</sup>

When he looked back on it in later years, this was a deeply ambiguous period in his political life. On the one hand, he felt that he was participating in an affirmative, grass-roots movement: distributing leaflets outside factory gates and

<sup>1</sup> "Interview with E. P. Thompson," *Radical History Review*, 3 (Fall 1976): 10–11.

<sup>2</sup> E. P. Thompson and T. J. Thompson, eds., *There Is a Spirit in Europe: A Memoir of Frank Thompson* (London, 1947), 11; and E. P. Thompson, "The Nehru Tradition," in E. P. Thompson, *Writing by Candlelight* (London, 1980), 138.

<sup>3</sup> "Interview with E. P. Thompson," 11.

<sup>4</sup> The term is Thompson's. As Eric Hobsbawm later wrote, "For those [Communists] not already in academic posts before the Cold War blacklisting began in the late Spring of 1948, the chances of university teaching were to be virtually zero for the next ten years"; Hobsbawm, "The Historians' Group of the Communist Party," in Maurice Cornforth, ed., *Rebels and Their Causes: Essays in Honor of A. L. Morton* (London, 1978), 25.

<sup>5</sup> Interviews with E. P. Thompson, Worcester, England, August 5 and November 10, 1986.

housing estates, getting directly involved in the working-class politics of his local area. On the other hand, Thompson repeatedly found himself clashing with the manipulative tactics of party leaders in London who wanted to extend their control over ever-broader sections of the noncommunist peace movement. He had seen longstanding members of the Labour Party face expulsion rather than give up their peace activism, and it galled him to see the Communist Party trying to control these independent-minded individuals from the outside.<sup>6</sup>

He nevertheless stifled his doubts and resentments in the name of loyalty to the broader cause. His first historical work, a biography of William Morris, offered a far-ranging reappraisal of the radical social critique embedded within Britain's Romantic heritage; yet it was also in many ways an obedient expression of the Communist Party line, the appropriation of Morris's life by a "true believer" whose political value judgments intruded regularly into the biographical narrative: "Twenty years ago even among Socialists and Communists, many must have regarded Morris's picture of 'A Factory as It Might Be' as an unpractical poet's dream: today [in 1955] visitors return from the Soviet Union with stories of the poet's dream fulfilled . . . Were William Morris alive today, he would not look far to find the party of his choice."<sup>7</sup> Thompson's indignation against the party's dogmatic and authoritarian methods did not break through to the surface until 1956, after Nikita Khrushchev's "Secret Speech" in February and the Soviet invasion of Hungary in November. Together with another young historian, John Saville, he borrowed a duplicating machine and began publishing a dissident Communist journal, *The New Reasoner*; he and Saville joined some 7,000 others—roughly a fifth of the party membership—in resigning from the party during that tumultuous fall.

For Thompson, the events of 1956 marked the beginning of a long pilgrimage between the two rival blocs of the Cold War. His disillusionment with Soviet-style Stalinism did not lead, as it did with some ex-Communists, toward a drastic ideological reversal, transforming him into an all-out defender of capitalism and the West. Instead, he began to campaign with equal fervor against the policies and ideologies of both superpowers, seeking to undermine the rigidly bipolar system of politics that their rivalry had created. After 1958, he participated in the Campaign for Nuclear Disarmament (CND), sharing in the daily practice of fund raising and organizing marches, as well as in the writing of articles to help define the campaign's wider goals and strategy. In 1959, *The New Reasoner* merged with another journal to form *The New Left Review*, and Thompson became an active

<sup>6</sup> *Ibid.*

<sup>7</sup> E. P. Thompson, *William Morris: Romantic to Revolutionary* (London, 1955), 795, 760. With hindsight, of course, one can already discern in this biography many of the subterranean tensions that would lead to Thompson's break with Stalinism in 1956. At the heart of the book lay Thompson's desire to rehabilitate "poetry," "sentiment," and the importance of each individual's moral courage within a Marxist culture that recognized only the iron discipline of the party, the "scientific" laws of economic change, and the mechanical contradictions among faceless classes. Morris's struggles with Marxist dogmatism and economism gave Thompson a chance to explore some of his own misgivings, while displacing them onto a historical discussion of events long past. It was not until the 1976 edition, however, in which Thompson excised the intrusive posturing of the 1950s, that reviewers began to acknowledge this book as a masterful biography. See, for instance, Edmund Penning-Rowsell, "The Remodeling of Morris," *Times Literary Supplement* (August 11, 1978): 913–14; and Peter Stansky, "The Protean Victorian," *New York Times Book Review* (May 15, 1977): 7, 48.



presence both in the *Review's* pages and on its editorial board. At the same time, he was still working as a teacher in Halifax, he threw himself into the work of promoting New Left Clubs in as many British cities as possible, and he had embarked, in the late 1950s, on the historical work that was to render him internationally famous, *The Making of the English Working Class* (published in 1963).<sup>8</sup>

Not surprisingly, the theoretical concerns that motivated Thompson in *The Making of the English Working Class* were intimately related to his political work in the New Left. Indeed, it is possible to say that his questions of theory and his questions of practice were identical: How do people change the structures of the society in which they live? How does a "new consciousness" emerge among specific groups of people, and how is it possible for this "new consciousness" to bring about the transformation of a society?

In his research on the nineteenth century, Thompson had discovered that "the most revolutionary 'shock troops' of the working class were [often] not factory proletarians at all [as Marx had assumed] but were the depressed handworkers; while in many towns, including large industrial towns, the actual nucleus of the labour movement was made up largely of artisans—shoemakers, saddlers and harnessmakers, building workers, booksellers, small tradesmen, and the like. Further, so far from being vacillating 'petit-bourgeois' elements, these were often . . . among the most consistent and self-sacrificing participants in the working-class movement."<sup>9</sup> Not only was the nineteenth-century working class far more heterogeneous than Marx had conceived it to be, but the forging of a sense of unity among these disparate groups had hinged crucially on the organizational efforts of determined individuals. Class unity, far from being an automatic outcome of a particular mode of production, consisted in a new way of perceiving oneself in relation to others: a self-image painfully built up and refined through a long process of tentative advances and humiliating failures. Moral courage, creative reflection on one's environment, difficult choices in the shifting of one's allegiances—these were among the key factors in the making of class consciousness.

This re-emphasis on culture and human initiative in the shaping of working-class history went hand in hand with a revised notion of revolution. Thompson utterly rejected the elevation of the 1917 Russian Revolution to the status of a general model or schema for all "transitions to socialism." In a lengthy article titled "Socialist Humanism" (written in 1957), he attacked contemporary Soviet Communism as "anti-democratic, inherently bureaucratic, alternately paternalist or despotic towards the people."<sup>10</sup> He painstakingly analyzed this form of statism, tracing its roots not only to Stalin himself but also "to ambiguities in the thought of Marx and, even more, to mechanistic fallacies in Lenin's writings."<sup>11</sup>

<sup>8</sup> E. P. Thompson, *The Making of the English Working Class* (New York, 1963). For discussions of the book's impact on subsequent social history, see Bryan D. Palmer, *The Making of E. P. Thompson: Marxism, Humanism, and History* (Toronto, 1981), 65 and following; and Harvey J. Kaye, *The British Marxist Historians* (Cambridge, 1984), 172 and following.

<sup>9</sup> E. P. Thompson, "Revolution Again," *New Left Review*, 6 (November–December 1960): 24.

<sup>10</sup> E. P. Thompson, "Socialist Humanism," *New Reasoner* (Summer 1957): 131.

<sup>11</sup> See Thompson, "Socialist Humanism," 132–33. At the core of the problem, he saw a failure by Marx and Engels to understand fully, or to emphasize adequately, "how men's ideas were formed, and wherein lay their field of agency." The crucial ambiguity lay in their use of the pivotal word



Thompson insisted, however, that a strong humanist element in the Marxist tradition was still alive and could be separated from the disastrous dogmatism and authoritarianism with which it had been fused. Steering between the reformist model of the Labour Party and the cataclysmic model of orthodox revolutionaries, he sought to define a concept of revolution that would remain true to the values of "socialist humanism." This revolution would be gradual and nonviolent, basing its advance on "unrelenting reformist pressures in many fields" and on voluntary changes in people's values;<sup>12</sup> yet it would be a "revolution" nonetheless, because it would aim not at any accommodation with acquisitive capitalism but at a thoroughgoing transformation into a society in which the logic of community and of reciprocity prevailed over competitive individualism.

This conception of revolution provoked much criticism among those on the left who were used to thinking in terms of physical force as the ultimate arbiter in the struggle between classes. As early as 1958, Thompson's "socialist humanism" had been criticized by fellow contributors to *The New Reasoner* as "fitting on the perimeter of idealism" and as the work of a "utopian socialist" who believed too strongly in "the power of the word."<sup>13</sup> How, his many critics asked, could this idea of peaceful revolution have any efficacy within an affluent society in which the ideological hegemony of the established order was notoriously powerful and resilient? What would this "revolution" look like?<sup>14</sup>

Thompson's answer hinged on a historical analogy. Just as in the early nineteenth century, when the determined agency of Chartist organizers had helped to produce an intense collective awareness of common interests and aims, so in the mid-twentieth century a new movement for popular self-organization could make all the difference. "The problem," he wrote in 1960,

is not one of "seizing power" in order to create a society in which self-activity is possible, but one of generating this activity now within a manipulative society . . . [One] determined municipal council, probing the possibility of new kinds of municipal ownership in the face of Government opposition; one tenant's association with a new dynamic, pioneering on its own account new patterns of social welfare—play-centers, nursery facilities, community services for and by the women—involving people in the discussion and solution of problems of town planning, racial intercourse, leisure facilities; one pit, factory, or sector of nationalized industry where new forms of workers' control can actually be forced upon management . . . a break-through at any one of these points would immediately help in precipitating a diffuse aspiration into a positive movement.<sup>15</sup>

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"reflection," which could either mean a passive echoing, like a mirror, or a much more active and creative process of reworking given information into a new synthesis. The gradual erosion of the second meaning in the transition from Marx to Lenin to Stalin had ultimately resulted in a rigid and brutal ideological orthodoxy intrinsically hostile to popular initiative and the genuine renewal of ideas. "How much easier," he wrote ironically, "if the people had no minds, if the 'superstructure' was cut out and society was all 'base': then this clumsy business of reflection could be done away with"; "Socialist Humanism," 112.

<sup>12</sup> E. P. Thompson, "Revolution," *New Left Review*, 3 (May–June 1960): 7.

<sup>13</sup> See the summary of these criticisms in E. P. Thompson, "Agency and Choice," *New Reasoner* (Summer 1958): 6.

<sup>14</sup> See the essays and correspondence published in response to Thompson's article "Revolution," in *New Left Review*, 4 and 5 (July–August and September–October 1960).

<sup>15</sup> Thompson, "Revolution Again," 31.

Here lay Thompson's hopes for the New Left. It would bring together ex-Communists with the left wing of the Labour Party, nuclear disarmament activists with trade union members, dissidents in the Eastern bloc with Western rebels against NATO, offering an international grass-roots alternative to the "sterile antagonisms" of state capitalism and state Communism.<sup>16</sup> He was not sanguine about the prospects for success and singled out the ideological hardening of the Cold War as perhaps the greatest obstacle.<sup>17</sup> Nevertheless, he plunged into his work in the New Left Clubs and CND with an ardor that could only have been fueled by genuine optimism as well as political conviction.

In the end, however, Thompson's hopes for the early 1960s as a potential watershed proved unrealizable. CND, riven by internal dissension over the tactics of protest, gradually began to lose its élan. The goal of unilateral nuclear disarmament, which had provided an effective rallying point for the campaign's diverse supporters, failed to infuse the movement with a sense of long-range political direction.<sup>18</sup> The New Left unraveled for some of the same kinds of reasons. By the late 1960s, as various New Left factions looked back on the decade's disappointments, each group offered its own explanation of past failures—from the absence of a genuine base among working-class trade unions to an excessive absorption with CND, from the lack of a cogent revolutionary strategy to an overly intellectualized conception of class struggle.<sup>19</sup> Although all these interpretations probably contained elements of truth, it was precisely this acute divisiveness among the New Left's constituencies, and their fratricidal competitiveness, that most seriously undermined its collective efforts.<sup>20</sup>

<sup>16</sup> E. P. Thompson, "The New Left," *New Reasoner* (Summer 1959): 7.

<sup>17</sup> See, for instance, Thompson, "The New Left," 17; and "Socialist Humanism," 138.

<sup>18</sup> See Peggy Duff, *Left, Left, Left* (London, 1971), 130–31.

<sup>19</sup> For a sampling of these views, see Perry Anderson, "The Left in the Fifties," *New Left Review*, 29 (January–February 1965): 3 and following; E. P. Thompson, "The Peculiarities of the English," in John Saville and Ralph Miliband, eds., *Socialist Register, 1965* (New York, 1965), reprinted in E. P. Thompson, *The Poverty of Theory and Other Essays* (New York, 1978), 245–301; David Widgery, ed., *The Left in Britain, 1956–68* (Harmondsworth, Eng., 1976); Ken Coates, "How Not to Reappraise the New Left," in John Saville and Ralph Miliband, eds., *The Socialist Register, 1976* (London, 1976), 111 and following.

<sup>20</sup> Thompson himself came to be embroiled in a particularly bitter feud early in 1962, a conflict that led to his withdrawal from the *New Left Review* the following year. The immediate cause of the rift lay in the appointment of a new editor, Perry Anderson, who represented a younger group of New Left thinkers. "We freely spoke of the 'Old Guard' among ourselves," Anderson later wrote, "at a time when Thompson was just over 35"; Perry Anderson, *Arguments within English Marxism* (London, 1980), 137. This split between the so-called "Old New Left" and "New New Left" rested not only on differing generational experiences but also on profoundly divergent theoretical orientations. To Anderson and his group, the events of 1956 and "socialist humanism" seemed far less important than the new political movements taking shape within the Third World and the new currents of existentialist Marxism developing in Paris. Jean-Paul Sartre and Frantz Fanon were the exciting intellectual figures, in comparison with which William Morris seemed provincial and obsolete. To the "Old Guard"—the Thompsons, John Saville, Mervyn Jones, Raymond Williams, Ralph Miliband, and others—this new direction taken up by Anderson seemed more like a disturbing mixture of hyper-theoretical virtuosity and anti-Western *machismo* (E. P. Thompson, unpublished manuscript, 1962). Early in 1963, the Thompsons and the rest of the "Old Guard" resigned from the *Review's* editorial board, leaving the field open for Anderson to undertake a full-scale reorientation of its content and style.

IN HIS SUBSEQUENT POLITICAL ACTIVISM, Thompson continued to refer to himself as a "dissident Communist" and, still later, as a "libertarian Communist."<sup>21</sup> He refused to accept the equation, "Communism = Soviet totalitarianism" and insisted that the genuine roots of the Communist movement lay in the struggles for democratic self-rule among the workers of the early nineteenth century.<sup>22</sup> While ardently espousing his own brand of "peaceful revolution," therefore, Thompson saw no contradiction in joining the Labour Party—which he and his wife Dorothy Thompson did in 1962.<sup>23</sup> During the late 1960s, he strongly supported the students and counterculture activists who were rebelling against the Vietnam War; yet he deplored their confrontational tactics and "punch-ups" with the police. Too many of their actions struck him as "irrationalist, self-exalting gestures of style," and he could not share their belief "that some campus-based revolution would by-pass the working class."<sup>24</sup>

Only a few years later, however, Thompson became a central participant in a different student movement. In 1965, he and Dorothy Thompson had moved south from Halifax to Warwick, where he had been asked to head the Centre for the Study of Social History at the newly formed Warwick University. During his fifth year of teaching there, in 1970, students began to agitate for greater democracy in the educational process; on the night of February 11, they occupied the campus administration building. Thompson, who "had been arguing for a more reformist course of action," was telephoned at 8:30 in the evening by students who had opened confidential university files.<sup>25</sup> They had found documents clearly establishing that university officials were monitoring the political activities of students and staff, screening prospective students according to their political views, and had apparently sought to secure the deportation of labor historian David Montgomery, who was visiting from the United States.<sup>26</sup> Thompson responded by photocopying the key documents and distributing them to all faculty members the following morning. In the furor that ensued, Thompson defended his publicizing of the documents on the grounds that the information

<sup>21</sup> Interviews with E. P. Thompson, Worcester, England, August 5 and November 10, 1986. This tenacious adherence to the political label of "Communist" was possibly due more to a sense of loyalty and tradition than to any substantive correspondence with contemporary ideological alignments. Unlike the vast majority of the world's self-proclaimed Communists in the late twentieth century, Thompson rejected the primacy of economic causes in historical process and expressed a marked antipathy to the ideas of violent revolution, dictatorship of the proletariat, and the leading role of the party. To an outsider, therefore, his political position might justifiably appear closer to the label of "social democracy" than to that of "Communism"—although in reality, his appeal for direct popular self-government placed him equally far from the state-centered traditions of both. For a discussion of Thompson's views on apostasy, see his "Open Letter to Leszek Kolakowski," reprinted in *The Poverty of Theory*, 303–402.

<sup>22</sup> "E. P. Thompson: Recovering the Libertarian Tradition," interview, *Leveller*, 22 (January 1979): 21.

<sup>23</sup> "Our first application to join the Labour Party was turned down by a high-level screening committee, which interviewed us and demanded that we say if we were Marxists or not. The Halifax Labour Party protested against our rejection and eventually admitted us"; E. P. Thompson to Michael Bess, April 2, 1989.

<sup>24</sup> "Interview with E. P. Thompson," *Radical History Review*, *op. cit.*, 9; interviews with E. P. Thompson, Worcester, England, August 5 and November 10, 1986.

<sup>25</sup> *Ibid.*

<sup>26</sup> See E. P. Thompson, ed., *Warwick University Ltd.: Industry, Management and the Universities* (Harmondsworth, Eng., 1970), chap. 6.

they revealed required a full-scale inquiry into the university's administration.<sup>27</sup> Having sided with the students, he found himself caught between two kinds of attack: liberals and conservatives denounced his breach of the university's right to privacy while militant leftists ridiculed his effort to restore impartiality to the university's official procedures. This impartiality, Thompson argued, was not to be dismissed as a mere sham but represented a crucial principle that one should fight to uphold. "It is here," he wrote, "that red moles and Maoists, looking down at our contemptible liberal maunderings from the revolutionary heights of North London, tend to go wrong. In so far as they persuade students that *all* the rules, the democratic and constitutional defenses, of the institution are no more than liberal 'masks,' their analysis could be self-fulfilling . . . They can't denounce democracy as a spoof and at the same time help to man its defenses."<sup>28</sup>

Eventually, after an official inquiry and modest reforms, the tumult at Warwick University subsided; yet the impact of this episode on Thompson's thought remained significant. Throughout the following decade, Thompson produced a steady series of articles defending the institutions of British liberalism from what he saw as an array of encroachments.<sup>29</sup> On the left, he observed a tendency to dismiss "bourgeois legality" as a mere façade behind which the manipulative rule of power and money went on unabated. On the right, he discerned a growing authoritarianism in British society, in the "Official Secrets Act," the political screening of juries, the spread of wiretapping, and the manufacture of consensus by "a compliant press [and] a managed television."<sup>30</sup> Both these tendencies, left and right, contributed to an erosion of precious civil liberties—liberties rooted in historic struggles for democratic self-rule and without which his own vision of a new democratic spirit would become meaningless.

Thompson's historical research in these years closely reflected this preoccupation with the role played by law and by public perceptions of constitutional rights. In *Whigs and Hunters* (1975), he came to a startling conclusion (for a self-proclaimed Marxist): the law, in eighteenth-century England, had served simultaneously as an oppressive instrument of class rule *and* as a valuable constraint on the naked exercise of power. Rulers and ruled had certainly stood at opposite ends of society, but they had also struggled within a common (or overlapping) medium of moral values and legal institutions that provided costs and benefits for both sides. "The rhetoric and the rules of a society," he wrote, "are something a great deal more than sham. In the same moment they may modify, in profound ways, the behavior of the powerful, and mystify the powerless. They may disguise

<sup>27</sup> Thompson, *Warwick University Ltd.*, chap. 7. This argument was in fact inconsistent with the liberal principles that Thompson sought to defend. Later in the decade, he would bitterly oppose any attempt by the state to use illegally obtained information in prosecuting criminal suspects; yet in this case he was willing to use the illegally obtained documents from the university files to launch an investigation of its administrators. Thompson had been placed in a classic dilemma on the evening of February 11, in which he was forced to violate some aspect of his own liberal values either by taking action or by remaining silent. It is arguable that his choice to proceed with the publication of the documents represented a basic error of judgment—for it contradicted the underlying principles that Thompson would passionately defend throughout the following years.

<sup>28</sup> Thompson, *Warwick University Ltd.*, 151–52.

<sup>29</sup> Many of these articles were subsequently gathered into a single volume, *Writing by Candlelight* (London, 1980).

<sup>30</sup> E. P. Thompson, "A State of Blackmail," in *Writing by Candlelight*, 133.

the true realities of power, but, at the same time, they may curb that power and check its intrusions."<sup>31</sup> This was far from providing a sanguine vision of social conflict, in which the reconciliation of opposed interests lay within easy reach. Nevertheless, it proposed a conceptual framework in which rulers and ruled were also subtly interdependent and could struggle to alter the mutual norms of tacit social consent that bound them together. In *Whigs and Hunters*, the domain of culture became a battleground in which crucial struggles could be fought out—struggles in which the dominance of one social group over others could be challenged, undermined, or even overthrown. So innovative (and so un-Marxist) was this understanding of law that it prompted another socialist historian, Perry Anderson, to wonder: "Does this . . . signify a movement to the right by Thompson?"<sup>32</sup>

It was precisely during the late 1970s, moreover, that Thompson undertook a full-scale reevaluation of what it meant to be simultaneously a Marxist and a professional historian. The immediate focus of his concern was the work of the French Marxist theorist Louis Althusser,<sup>33</sup> whose ideas Thompson regarded as both pernicious and alarmingly fashionable among growing numbers of leftist thinkers. His rebuttal of Althusser—an essay called "The Poverty of Theory"—took the form of a ribald (and often intemperate) Swiftian polemic, yet it also gave Thompson a chance to articulate his own ideas on "the historian's craft" and its contributions both to scholarly discourse and to concrete political practice.

At the center of "The Poverty of Theory" lay Thompson's rejection of the venerable Marxist distinction between economic "base" and cultural "superstructure." As early as 1957, he had already attacked the rudimentary version of this distinction put forth by Stalinists.<sup>34</sup> But Althusser, in Thompson's view, had done nothing to mitigate the dangerous reductionism of the Stalinist metaphysics; he had merely substituted a fancy form of machinery for the crude mechanism of the earlier model.<sup>35</sup> This line of thinking was profoundly dangerous, Thompson believed, because it dehumanized the historical process, severing the link between human agency and the larger processes of historical change. What were human beings, in Althusser's analysis, but the unwitting "bearers" of vast and impersonal economic forces over which they had little or no control?

As an alternative, Thompson proposed an entirely different conception of historical causation, in which individuals still played a significant role. Human lives, he argued, unfolded on many levels—economic, political, psychological,

<sup>31</sup> E. P. Thompson, *Whigs and Hunters: The Origin of the Black Act* (Harmondsworth, Eng., 1975), 265.

<sup>32</sup> Anderson, *Arguments within English Marxism*, 201.

<sup>33</sup> See, for instance, Louis Althusser, *For Marx*, Ben Brewster, trans. (London, 1970); and *Reading "Capital"* (London, 1970).

<sup>34</sup> See Thompson, "Socialist Humanism."

<sup>35</sup> Whereas the Stalinist had posited a blunt linear relation between economic cause and cultural effect, Thompson argued, Althusser had proposed an epicyclic model that Thompson likened to an orrery—a machine used by astronomers to illustrate the complex movements of the solar system. If one turned the handle, the moons began to revolve in tiny circles around their planets, which themselves revolved in larger circles around the unmoving sun. In precisely the same way, Thompson argued, Althusser's view of history assigned small circles of "relative autonomy" to cultural factors like religion, politics, and the arts; and all these lesser spheres were governed "in the last instance" by economic causal processes, located at the unmoving center of the machine.



biological—any one of which might provisionally assume causal priority in determining a given formation of events. These “levels” continually interacted with each other in ways that defied the traditional Marxist impulse to reduce all historical change to its economic determinants.<sup>36</sup>

By way of an example, Thompson spent several pages in “The Poverty of Theory” describing the imaginary case of a British woman who was a trade-union organizer in her factory, a Labour Party activist, a mother of three children, an amateur musician, a member of the Church of England, the wife of one man and the mistress of another. All these “levels” played crucial roles in determining her life; no single domain could adequately explain the distinctive story that her daily actions gradually formed. “Is the woman,” Thompson asked, “no more than a point at which all these relations, structures, roles, expectations, norms, and functions *intersect*; is she the carrier of all of them, simultaneously, and is she *acted* by them, and absolutely determined at their intersection?”<sup>37</sup> The only way to find out, Thompson argued, was not to consult some abstract Marxist model or sociological theory but simply to observe her history, which expressed the totality of the causal relations reverberating within her life. Thompson offered two possible scenarios for the woman’s story. In the first, she suffered a nervous breakdown under the conflicting pressures of her numerous allegiances and had to be kept going on tranquilizers. In the second, she went to her workplace and rebelled: “She shouts out: ‘I’m not a bloody THING!’ . . . She calls out the workshop on strike. She leaves her husband and she sacks her lover. She joins women’s lib. She leaves the Church of England.”<sup>38</sup> A human being, Thompson insisted, could not be reduced to abstract categories, no matter how intricate and subtle they might appear; out of the synthesis that each individual continually created among the structures that impinged on his or her life, a unique and ultimately unpredictable story arose. Above all, an individual human being possessed the potential for taking the initiative and changing the structures. Thompson believed that, like the woman in his story, every human being shared in the double-sided possibility of either succumbing to society’s complex pressures or rebelling and seeking to master them.

But how could a mere individual hope to prevail against the powerful structures of contemporary society? Here, a second key concept came into play for Thompson: the intrinsic circularity or reflexivity of all social behavior.<sup>39</sup> Since human beings were the stuff of which society was made, they changed themselves in the very process of transforming society; their role was simultaneously active and passive. Any action by an individual within a social context (insofar as it influenced or changed that social context) ultimately rebounded on that same individual’s life and actions; for the existence of every individual was deeply conditioned by the social whole of which he or she was a part, and any changes in that social whole must in turn influence all its constituent members. By changing social reality in however small a way, therefore, a single person could commen-

<sup>36</sup> See E. P. Thompson, “The Poverty of Theory,” in *Poverty of Theory*, 150–52.

<sup>37</sup> Thompson, “Poverty of Theory,” 150–52.

<sup>38</sup> Thompson, “Poverty of Theory,” 150–52.

<sup>39</sup> See, for instance, Thompson, “Peculiarities of the English,” 288 and following.



surately alter the manner in which other individuals perceived their environment and chose to act. These new perceptions could gradually add up, as increasing numbers of persons made them their own and conveyed them in turn to still larger numbers. Over extended periods, what was collectively deemed possible or impossible, desirable or undesirable, might shift substantially as a result of ideas initiated by relatively few persons. Thus an individual's impact might at first seem rather small and insignificant, but, through the circular dynamic of human culture, it could snowball into major structural changes.

However, Thompson pointed out, this form of power also possessed inherent limits; for the social context inevitably interfered with each individual's efforts to attain goals. Within the broader society, where many persons' wills and exertions crisscrossed, conjoined, or canceled each other out, the ultimate result was always a collective outcome that no single individual had quite predicted or intended. Thompson referred to this dual quality of social process as "the crucial ambivalence of our human presence in our own history, part-subjects, part-objects, the voluntary agents of our own involuntary determinations."<sup>40</sup>

Human agency, therefore, was like a form of play taking place within limits—limits whose evolution the players could consciously influence over time. Each individual possessed a finite yet open-ended opportunity to contribute to the shaping of culture. The collective result would be something different than any individual had envisioned, yet each could participate to some degree in determining the overall direction of social change. This was the precious space of partial free agency that Thompson never ceased in his writings to explore and to emphasize.

THOMPSON'S SECOND SOURCE OF OUTRAGE in "The Poverty of Theory" was a growing intellectual fashion he discerned among many of his scholarly contemporaries—a tendency to regard historical knowledge with a certain bemused contempt. In one passage, for instance, he indignantly described his recent encounter at Cambridge with a group of theoretically oriented anthropologists who had openly derided the "old-fashioned" empiricism of professional historians.<sup>41</sup> In response, he spent many pages defending the ideal of historical "objectivity" and seeking to reassert a chastened and self-critical epistemology of realist empiricism: "May not the object (real history) still stand in an 'objective'

<sup>40</sup> Thompson, "Poverty of Theory," 88. Thompson quoted a famous passage from *The Dream of John Ball* by William Morris to illustrate what he meant: "I pondered all these things, and how men fight and lose the battle, and the thing they fought for comes about in spite of their defeat, and when it comes turns out not to be what they meant, and other men have to fight for what they meant under another name"; William Morris, *The Dream of John Ball* (1886), quoted in "Poverty of Theory," 88. The work of Giambattista Vico had first alerted Thompson to this aspect of historical process, in which human actions appeared simultaneously as partly free and partly unfree, and it became increasingly clear to him that no metaphor (like "base and superstructure") could adequately express this self-referential dialectic of cultural change. "The difficulty," he wrote at one point, as he reflected over the possibility of a vegetation metaphor, "is not that a tree cannot think but that, if it could think, its thinking could not change—however imperceptibly—the soil in which it is rooted"; Thompson, "Peculiarities of the English," 289.

<sup>41</sup> Thompson, "Poverty of Theory," 37.

(empirically-verifiable) relationship to its knowledge?"<sup>42</sup> It is doubtful that Thompson's epistemological arguments in "The Poverty of Theory" would satisfy a rigorously postmodernist critic, for they did not explicitly grapple with the problems brought on by the "linguistic turn" in philosophy, nor did they pay much heed to such influential contemporary concepts as the Derridean "meta-physics of presence," the Foucaultean "episteme," or other related critical ideas.<sup>43</sup> For our present purposes, however, Thompson's empiricist arguments about objectivity are particularly interesting because they provide a revealing glimpse into how he balanced his own roles as historian and as political activist.

For Thompson, the essence of historical analysis lay in a "dialogue with the evidence"—a dialectic between the mind of the historical observer and concrete artifacts from the physical and textual record of the past.<sup>44</sup> Thompson offered several warnings about the form of knowledge that this process created. First, the nature of the original evidence was itself intrinsically problematic, since it came from perspectively limited and otherwise potentially misleading sources. Second, the resultant interpretation would necessarily remain both provisional and selective, since the process of testing and revision could never be truly complete. Finally, the historian's hypothesis could not be proven true but could only rest on the more shaky knowledge that it had not yet been proven false.<sup>45</sup>

Still, Thompson argued that this methodological foundation did allow historians to offer satisfactory explanations about the causes and connections of events. He saw no reason why rival explanations could not be fruitfully compared with each other, with an eye both to their internal coherence and to the way in which they confronted "fresh and inconvenient evidence."<sup>46</sup> This kind of testing would not provide access to any absolute or final form of "truth," but it would certainly allow historians to compare differing historical accounts and, in most cases, to adjudicate between them.

This was the historian's "discourse of the proof," and all historians, whatever their philosophical or ideological bent, could be "brought to trial" before it. Thus, Thompson wrote, "a feminist historian will say, or ought to say, that this history-book is wrong, not because it was written by a man, but because the historian neglected contiguous evidence or proposed conceptually-inadequate questions: hence a masculine 'meaning' or bias was imposed upon the answers. It is the same with the somewhat intemperate arguments which I and my Marxist

<sup>42</sup> Thompson, "Poverty of Theory," 19.

<sup>43</sup> This is not the place for an extended discussion of the complex philosophical questions underlying Thompson's empiricist stance. For provocative recent discussions of these methodological issues, see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988); and the account of the debate stirred up by Novick's book, in Karen J. Winkler, "Challenging Traditional Views, Some Historians Say Their Scholarship May Not Be Truly Objective," *Chronicle of Higher Education* (January 16, 1991).

<sup>44</sup> See Thompson, "Poverty of Theory," esp. 37–50.

<sup>45</sup> Thompson compared historical knowledge to scientific knowledge in this regard, implying that the natural sciences, unlike the humanities, *could* offer positive proof for their knowledge claims. Thompson apparently misunderstood the fact that most philosophers of science have long since abandoned this stronger epistemological claim for the natural sciences as well. See the discussion in "Poverty of Theory," 39–40.

<sup>46</sup> Thompson, "Poverty of Theory," 43.

colleagues often provoke within the academic profession."<sup>47</sup> To be a Marxist historian, for Thompson, meant that one's initial hypotheses would rely on a loose body of concepts—such as class, exploitation, the obfuscatory role of religion—which one would seek to "fit" with concrete evidence.<sup>48</sup> If historians found inconvenient or contradictory evidence, they should immediately begin to call into question the initial hypotheses and revise or discard them.

Thompson's willingness to subject Marxist doctrine to critical scrutiny, and to cast aside certain central tenets while modifying others, earned him as many critics on the left as he had already encountered on the right.<sup>49</sup> It would nevertheless be misleading to portray Thompson as invariably treading a "middle road" of critical even-handedness; for he also possessed a second voice, far sharper and more pugnacious, which he reserved for his political and ideological polemics. An interviewer for the *Radical History Review* called attention to this dualism in Thompson's character when he asked Thompson in 1976 whether the roles of historian and polemicist could be successfully reconciled. "The historian may tend to be a bit too generous," Thompson replied, "because a historian has to learn to attend and listen to very disparate groups of people and try and understand their value-system and their consciousness. Obviously in a very committed situation you can't always afford that kind of generosity. But if you afford it too little then you are impelled into the kind of sectarian position in which you are repeatedly making errors of judgment in your relations with other people."<sup>50</sup> The tension between these two stances recurred like a leitmotif throughout Thompson's career. On the one hand lay the ideal of open-minded receptivity to the diversity of experience—the stance that Thompson identified with "generosity" or "listening." On the other lay an equally important ability to take sides, however provisionally, and to intervene with concrete actions in the political life of one's society.

Thompson would no doubt have agreed that these two stances were in many ways mutually exclusive, but he reconciled them in daily practice by thinking of them as distinct phases in a broader process. In the "listening" phase, one sought (to the best of one's ability) to set aside provisionally preconceptions and personal values. When confronting historical documents or engaging in conversation with another person, a historian strove to empathize with the experiences of the "Other," to see events as much as possible from the perspective of the "Other." Then, in the "committed" phase, one returned to the familiar, personal perspective and submitted the documents or the opinions of the "Other" to one's own judgments and standards. In this second phase, it was perfectly appropriate to apply one's own values in assessing the moral or political import of past events; it was equally appropriate to intervene in contemporary political debates with an eye to realizing partisan aims. By moving back and forth between these two stances, an individual could hope to meet the demands for relative detachment

<sup>47</sup> Thompson, "Poverty of Theory," 41.

<sup>48</sup> Thompson, "Poverty of Theory," 45–46.

<sup>49</sup> For a summary of these criticisms, see Kaye, *British Marxist Historians*; Palmer, *Making of E. P. Thompson*; and Anderson, *Arguments within English Marxism*.

<sup>50</sup> "Interview with E. P. Thompson," *Radical History Review*, *op. cit.*, 17.

and impartiality that ideally characterized a historian while also engaging with contemporary issues as a hard-bitten polemicist.

Thompson himself never explicitly set forth this "two-phase" model, but in "The Poverty of Theory," while discussing the proper place of partisan values in historical discourse, he alluded to precisely such a notion: "Only we, who are now living, can give a 'meaning' to the past. But that past has always been, among other things, the result of an argument about values. In recovering that process, in showing how causation actually eventuated, we must, insofar as the discipline can enforce, hold our own values in abeyance. But once this history has been recovered, we are at liberty to offer our judgment upon it."<sup>51</sup> Implicit here was a complex set of intellectual procedures. First, the historian had a responsibility to become aware, in the most systematic way possible, of his or her own theoretical assumptions and value judgments. Since these assumptions and values often resided at an unconscious level, one faced an unending struggle to identify and to clarify them through a continual process of self-questioning. Having ascertained (provisionally) some of the main elements that brought structure to one's own intellectual perspective, one then needed to assess how these assumptions might limit questions or channel interpretations along some predetermined line. The final step would then require a systematic effort to dig up precisely the sorts of evidence that might support alternative theories or points of view. This conscientious attempt to disprove the initial hypotheses and explore opposing interpretations of the evidence constituted the essence of empirical testing. The result would not be "neutral" or "impartial" knowledge—for this lay beyond the grasp of any human being—but rather something much more limited: a chance of catching oneself in some of the grosser instances of tinting the picture of the past according to one's own colors.

Weighing the merit of rival historical interpretations, according to this principle, would require a comparison of the self-scrutiny and self-discipline that each author had applied (or failed to apply) during the process of research and writing. Obviously, such self-scrutiny provided only a limited corrective, but Thompson could still draw a meaningful distinction between this relatively circumspect phase of analysis and the overtly partisan reading of the past that he advocated in the "committed" phase. In practice, to be sure, the two stances of "listening" and "commitment" alternated and overlapped in subtle ways that defied any straightforward characterization; they were not always as easy to pinpoint and exchange as a swapping of hats. Nevertheless, the distinction between them allowed Thompson to argue that political commitment would not necessarily vitiate one's judgment as a historian.

As for the underlying aims of historical writing, they were twofold (in Thompson's view) and derived from the same tacit distinction between "listening" and "commitment." First, the aim of history writing was simply to provide the most accurate and comprehensive account of past events—in Thompson's own words, "recovering [the historical] process, . . . showing how causation actually eventuated."<sup>52</sup> The second aim, however, was explicitly political: one studied the

<sup>51</sup> Thompson, "Poverty of Theory," 42.

<sup>52</sup> See Thompson, "Poverty of Theory," 42–43.

actions and choices of past individuals in order to clarify (by analogy) the political and moral questions facing active citizens in the present. "If we succeed," he wrote in *The Poverty of Theory*, "then we reach back into history and endow it with our own meanings: we shake Swift by the hand. We endorse in our present the values of Winstanley, and ensure that the low and ruthless kind of opportunism which distinguished the politics of Walpole is abhorred."<sup>53</sup> An accurate historical account, in this reading, was not just intrinsically interesting, it also provided a chance for the author to reaffirm and to celebrate specific values for the audiences of the present day. It established a sense of precedent and tradition through which contemporary citizens might feel emboldened to make concrete moral and political choices in their own lives. Thus, in *The Making of the English Working Class*, Thompson had more in mind than just producing a reliable chronicle. "In some of the lost causes of the Industrial Revolution," he wrote, "we may discover insights into social evils which we have yet to cure . . . Causes which were lost in England might, in Asia or Africa, yet be won."<sup>54</sup> The coexistence of these two kinds of aims—accuracy and advocacy—did not trouble Thompson, for he believed that a conscientious writer could distinguish between the two and assign each to its own appropriate moment.

PERHAPS THE BEST ILLUSTRATION OF THOMPSON'S TWO PERSONAE—the polemicist and the relatively even-handed "listener"—lies in his work during the 1980s for European Nuclear Disarmament. Here, just as in his historical writing, Thompson did not pretend to take a "neutral" or "impartial" stand. Instead, he clearly identified himself as an independent writer on the left, while submitting his own views on the Cold War to the same standards to which he submitted his historical research. The result was a highly polemical perspective on contemporary history, in which Thompson fired at least as many salvos against the left as against the right. Soviet generals, Thatcherite bureaucrats, and his fellow Western Marxist intellectuals—all of them, in their turn—faced Thompson's criticism.<sup>55</sup>

The story of the END campaign is necessarily a complex one, for it transcended the scope of most preceding disarmament movements, spilling freely over national and ideological boundaries. At issue was the planned deployment by 1983 of new and more accurate missiles in NATO's arsenal—an act that Western governments regarded as a necessary response to new Soviet missiles in Eastern Europe. As this deadline approached, a powerful grass-roots campaign against both U.S. and Soviet missiles spread rapidly throughout much of Western

<sup>53</sup> Thompson, "Poverty of Theory," 42–43.

<sup>54</sup> Thompson, *Making of the English Working Class*, 13.

<sup>55</sup> See, for example, E. P. Thompson, "Notes on Exterminism, the Last Stage of Civilization," 2–3, 17–20; and "Europe: The Weak Link in the Cold War," esp. 330–45, both in *New Left Review*, ed., *Exterminism and Cold War* (London, 1982). In these articles, Thompson chided his fellow socialists on two serious counts. On the one hand, he wrote, some of them still tended to cling dogmatically to the idea that all the world's ills stemmed from capitalist "imperialism," while explaining away Soviet aggressions as "defensive over-reactions" and ignoring such untidy phenomena as the Sino-Soviet split or Polish *Solidarność*. On the other hand, he argued, their characteristic Marxist emphasis on economic factors led them to overlook the powerful irrational forces that fueled the Cold War at the levels of ideology, politics, and psychology.



Europe; peace demonstrations that had proudly boasted a few hundred individuals in the spring of 1980 swelled to some four million participants by the fall of 1983.<sup>56</sup>

Thompson's principal contribution to this burgeoning movement undoubtedly lay in his effort to politicize what had tended to be a primarily technical controversy, pushing public opinion beyond the prevalent obsession with adding and subtracting warheads toward a broader debate that embraced the basic political questions of the postwar period. He wrote dozens of articles for European and American newspapers, continually mailed off letters to the editor, and appeared often on British television, either in interviews or in news reports. He traveled to the major British cities, giving speeches and holding meetings (an average of ten public appearances per month between 1980 and 1982), and he went abroad on speaking tours from California to Hungary, Iceland to Greece, visiting some fourteen countries in all.<sup>57</sup> Wherever he went, Thompson hammered away at the same themes: the East-West rivalry had taken shape over the heads, as it were, of the Europeans whose continent it divided; now the time was ripe for the Europeans themselves to set in motion new historical forces that would bring the Cold War to an end.

Perhaps Thompson's greatest strength lay in the fact that he criticized both superpowers with equal vigor.<sup>58</sup> Unless one was determined *a priori* to portray Thompson as a Communist stooge or a covert CIA agent (both accusations were publicly leveled against him by NATO and Soviet apologists), it was nearly impossible to hear his speeches or read his articles without being struck by their painstaking even-handedness.<sup>59</sup> Together with Dorothy Thompson, he directly

<sup>56</sup> See, for instance, Ian Boyne, "400 March in Missile Protest," *Daily Telegraph* (April 8, 1980); Jane Dibblin, "An Outpouring of Protest—Peaceful and Determined," *END Journal*, 7 (December 1983–January 1984): 10–11; and Clive Rose, *Campaigns against Western Defense* (London, 1985), 157 and following. By the fall of 1982, a Gallup poll in England showed 58 percent of the population opposed to the basing of new American missiles. Similar polls throughout Western Europe showed comparably high rates of opposition: West Germany, between 25 percent and 67 percent (depending on sources); Italy, between 40 percent and 54 percent; the Netherlands, between 51 percent and 68 percent; and even in France (where no missile deployment was scheduled), a surprisingly high 44 percent. See Gregory Flynn and Hans Rattinger, eds., *The Public and Atlantic Defense* (London, 1985), 89, 134, 192, 256. See also David Fairhall and Martyn Halsall, "Majority Opposes Trident Missiles in Poll," *Guardian* (November 16, 1982). While the title of the article mentions only Trident missiles, the text gives figures for opposition to cruise missiles as well.

<sup>57</sup> Interviews with E. P. Thompson, Worcester, England, August 5 and November 10, 1986.

<sup>58</sup> Thompson articulated his ideas on the Cold War in a steady series of speeches, pamphlets, articles, and books that poured forth from his pen starting in 1980: "Protest and Survive" (April 1980), in E. P. Thompson and Dan Smith, eds., *Protest and Survive* (Harmondsworth, 1980); E. P. Thompson, "Notes on Exterminism, the Last Stage of Civilization" (May 1980), *New Left Review*, 121 (May–June 1980); *Beyond the Cold War* (New York, 1982); "Europe: The Weak Link in the Cold War," in *New Left Review*, ed., *Exterminism and Cold War* (London, 1982); *The Heavy Dancers* (London, 1985); *Double Exposure* (London, 1985); *Star Wars* (New York, 1985); Mary Kaldor and Paul Anderson, eds., *Mad Dogs: The US Raids on Libya* (London, 1986); and Dan Smith and E. P. Thompson, eds., *Prospectus for a Habitable Planet* (Harmondsworth, 1987).

<sup>59</sup> Many of Thompson's right-wing critics in the West have argued that this "even-handedness" merely represented a cynical feint on Thompson's part, designed to deflect the traditional accusation that the peace movement was "soft on communism" and guilty of "appeasement." Such an argument, however, could not be sustained without grossly distorting Thompson's writings and public statements; for he had established himself as a prominent critic of the Soviet Union ever since 1956, and his disillusionment had become even sharper after the suppression of the Prague Spring in 1968. He had also consistently backed up his words with deeds: on several occasions during the 1970s, for



took up this issue in an internal END letter circulated in 1980: "The point is that [some figures on the left] are trying to squash END policies down to a wholly anti-NATO stance, and to take out any anti-Soviet sting . . . But if we had believed that the 'fault' in the present build-up and war threat was entirely with one side in the great-power confrontation, then there would be no need for END . . . If our protest movement is to reclaim its right to defend civil liberties and the right to dissent, then it must be prepared to speak out about Eastern Europe."<sup>60</sup>

Thompson did not believe, however, that the two superpowers were "equivalent" to each other or that their roles in the arms race were necessarily the same. "I don't mean to argue for an *identity* of process in the United States and the Soviet Union," he told an audience in 1981, "nor for a perfect symmetry of forms . . . I mean to stress, rather, the *reciprocal* and interactive character of the process. It is in the very nature of this Cold War show that there must be two adversaries: and each move by one must be matched by the other. This is the inner dynamic of the Cold War which determines that its military and security establishments are *self-reproducing*. Their missiles summon forth our missiles which summon forth their missiles in turn. NATO's hawks feed the hawks of the Warsaw bloc."<sup>61</sup> This process was not, however, confined to the domain of military technology. It spilled over into politics, defining the agendas of statesmen according to preordained scripts that had been conceived long before. It distorted the economies of the superpowers, saddling the markets of the West and the bureaucracies of the East with a profound dependency on their ever-expanding military sectors. Finally, it intruded into the collective psychology of the two societies, wrenching all impulses toward diversity or spontaneity into the conformist channels of a single binary opposition.

In his speeches, Thompson repeatedly affirmed that a safe exit from this increasingly complex and uncontrollable process was still possible—but only if people acted quickly, before the historical tendencies had acquired an irreversible momentum. He advocated a twofold strategy, explicitly linking disarmament with the promotion of civil liberties. First of all, he argued, END activists had to push hard to establish contacts and regular channels of communication with like-minded persons across the Iron Curtain. They should uncompromisingly defend Eastern bloc dissidents and human rights activists, and they should also reach out to the less vocal sectors of the East European population, forming bonds with religious and youth groups, trade unions, scientists, or professional organizations. At the same time, END would seek to mobilize a powerful movement in Western

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instance, he had circulated petitions and given public speeches to protest the persecution of civil rights prisoners in the Soviet bloc. For examples of this line of right-wing criticism, see Paul Mercer, "The Thompson Technique," in Mercer, *"Peace" of the Dead: The Truth behind the Nuclear Disarmers* (London, 1986), 321 and following; Scott McConnell, "The 'Neutralism' of E. P. Thompson," *Commentary* (April 1983): 31; and Thomas M. Cynkin, "The British Antinuclear Movement," in James E. Dougherty and Robert L. Pfaltzgraff, Jr., eds., *Shattering Europe's Defense Consensus: The Antinuclear Protest Movement and the Future of NATO* (McLean, Va., 1985), 41 and following.

<sup>60</sup> Dorothy Thompson and Edward Thompson, "END—Retrospect and Next Steps" (August 18, 1980), unpublished letter, 10; in private papers of Mary Kaldor on END, Brighton, England.

<sup>61</sup> E. P. Thompson, "Beyond the Cold War," lecture delivered at Worcester Guild Hall on November 26, 1981; reprinted in *Beyond the Cold War*, 169 (emphasis in original).

Europe to stop escalation of the arms race, bringing great pressure to bear on both superpowers to halt deployment and begin removing their European-based missiles. Once these twin projects were under way, Thompson believed, they would strongly reinforce each other. A relaxation of international military tensions would remove the Soviet leadership's main excuse for its occupation of Eastern Europe, and any tangible steps toward liberalization in Eastern Europe would progressively mitigate the Western fears that provided NATO with its underlying rationale. A resurgent civil society might steadily push back (from both sides) the rigid institutions of a militarized society.

Here were the two concepts at the heart of "The Poverty of Theory," making their appearance in the political practice of the 1980s: the Cold War, for Thompson, could only be understood as a "multi-level" phenomenon, arising out of the mutual interaction of political, economic, cultural, and military factors. The way out of the Cold War, moreover, lay in human agency: for, in framing the goals of END, Thompson explicitly returned to the concept of popular self-assertion that he had first explored in *The Making of the English Working Class*. Just as the English workers in the early nineteenth century had rebelled against being turned into machine-like instruments of an exploitative social system, so the citizens of East and West might rebel today against the mindless social process that was carrying them collectively toward a thermonuclear war that no one individual wanted.

In the end, despite the peace demonstrations, NATO deployed its new missiles on schedule early in 1984, and the same missiles were later dismantled as part of the revolutionary changes in European politics that took place between 1987 and 1991. In 1989, the diplomatic correspondent for *Time* magazine, Strobe Talbott, offered an assessment of superpower relations during the preceding decade. "Only under pressure from across the Atlantic," he concluded, "did the Reagan Administration enter talks with the Soviets on intermediate-range missiles."<sup>62</sup> President Reagan himself later confirmed in his memoirs that the "Euromissile" controversy of the early 1980s had played a major role in shaping his proposals to the Soviets.<sup>63</sup> Thus, although this "pressure from across the Atlantic" certainly came from a far wider array of political forces than END, it would be hard to deny the long-term impact of the huge antinuclear rallies, with their banners proclaiming "No to the Blocs!"

<sup>62</sup> Strobe Talbott, "Back in Business," *Time* (June 12, 1989): 34.

<sup>63</sup> "My proposal of the zero-zero option," Reagan wrote, "sprang out of the realities of nuclear politics in Western Europe . . . Whipped up by Soviet propagandists, thousands of Europeans were taking to the streets and protesting the plans to base additional nuclear weapons in Europe, arguing that their presence would cause future nuclear wars to be confined to Europe . . . Helmut Kohl . . . told me during a White House visit that the propaganda offensive was becoming highly sophisticated and effective in convincing Europeans that the United States was a bloodthirsty, militaristic nation. This view of America shocked me: We were the most moral and generous people on earth, . . . and now we were being cast—effectively, Kohl said—as villains. It was clear we'd have to do a better job of conveying to the world our sense of morality and our commitment to the creation of a peaceful, nuclear-free world. After considerable discussion with the cabinet and our arms control experts, I decided to propose the zero-zero plan"; Ronald Reagan, *An American Life* (New York, 1990), 295–96; see also 552–59, 590–91.

HISTORIAN AND POLITICAL ACTIVIST: how successfully did Thompson wear these two hats? If political bias implies (among other things) a systematic suppression of "inconvenient" evidence or ideas, then anyone who seeks such bias in Thompson's major works is likely to come away frustrated; for although he clearly identified with the political left, he refused to engage in such suppression.<sup>64</sup> Like George Orwell, Thompson regularly criticized his own camp with the same pungency that he directed against his ideological enemies on the right, and he readily embraced ideas and hypotheses alien, and even contrary, to his own professed ideological tradition.<sup>65</sup> The primary relation between Thompson's activism and his historical research has lain at a deeper and subtler level.

Perhaps the most commonly recurring criticisms of Thompson's work have revolved around the question of his excessive optimism or "idealism." Critics such as Tom Nairn and Perry Anderson, for instance, have maintained that *The Making of the English Working Class* exaggerated the long-term gains won by the early labor movement.<sup>66</sup> Along similar lines, historian Michael Howard has argued that Thompson, as a peace activist, greatly overestimated the political maturity of Europe's peoples; his speeches and writings in END remained far too vague—if not downright sanguine—on the subject of keeping the peace in a Europe without superpowers.<sup>67</sup> What if a serious conflict emerged between two or more peoples, such as the Romanians and Hungarians, or the Poles and Germans—any of the hostile pairs that had confronted each other during Europe's long and bloody past? Who would arbitrate between them? Who would enforce the peace? These kinds of questions almost never arose in Thompson's writings; he appeared to pin his hopes on a somewhat precarious notion that the memory of past violence and the fear of a nuclear holocaust would bring forth an unprecedented rationality and self-control among the peoples of Europe.<sup>68</sup> Here, it seems, the blind spots in

<sup>64</sup> This generalization must of course exclude the 1955 edition of Thompson's *William Morris*, already mentioned above. It would take a second edition, produced in the 1970s, to "purge" the 1955 edition of its blatant political hectoring.

<sup>65</sup> Indeed, he included so much "inconvenient evidence" in *The Making of the English Working Class* that, according to Perry Anderson, Thompson's critics could construct a convincing counterargument to the book's main thesis by drawing on the wealth of examples presented within the narrative itself. See Anderson, *Arguments within English Marxism*, 30–49.

<sup>66</sup> Thompson, according to Anderson, overrated the revolutionary force unleashed by the newly made working class of the early nineteenth century; this represented only one instance of Thompson's broader tendency to inflate the role of human agency as against the forces of necessity—"to overpitch the share of conscious choice and action within the social formations of the past." Anderson, *Arguments within English Marxism*, 57. Anderson cited the grim conclusions reached by Tom Nairn to show that a far more pessimistic assessment was also possible: "The great English working class, this titanic social force which seemed to be unchained by the rapid development of English capitalism in the first half of the century, did not finally emerge to dominate and remake English society. It could not break the mold and fashion another. Instead, after the 1840s it quickly turned into an apparently docile class." Tom Nairn, "The English Working Class," *New Left Review*, 24 (March–April 1964): 43, quoted in Anderson, 44.

<sup>67</sup> See Michael Howard, *The Causes of Wars and Other Essays*, 2d edn. (Cambridge, Mass., 1983), chap. 1, see 116 and following.

<sup>68</sup> "It is quite possible that Europe will blow up before we succeed," Thompson wrote in 1984 to a member of the Czech human-rights group, Charter 77: "To achieve our common objective will require an unprecedented maturity in those popular movements, West and East, which seek to pace and monitor the transition, and to limit episodes of 'destabilization'"; E. P. Thompson, "The Two Sides of Yalta," in *Heavy Dancers*, 181. The key weakness in Thompson's position lay in precisely this point: his tendency to expect "unprecedented maturity" from the peoples of Europe, when

Thompson's historical research converged with those of his political activism: his favorable assessment of human agency in Europe's past went hand in hand with his hopeful proposals for Europe's future.

This recurring tendency in Thompson's work might best be understood as an articulation of faith. The writer Isaac Bashevis Singer, in an interview shortly before his death, was asked by a reporter if he truly believed that human beings possessed free will. "I have to believe this," Singer wryly replied: "I have no choice." In a similar way, Thompson's career appears to have rested on precisely such a tacit assumption: although it is not clear, at any given moment, to what extent human beings will be able to shape their own history, the space for meaningful action rests in part on this very hope. Without the sense of history as a field of alternative possibilities, in which individual and collective endeavors can make a decisive difference, the chances of significant human agency in the present must swiftly fade to zero.

Thus the strengths and the weaknesses of Thompson's work may well have sprung from a common source: his desire to substantiate and to broaden this sense of history, showing that persons in the past, as well as contemporary citizens, could make an impact on the events of their time. The drawback of such a stance is that it tends toward a systematic overestimation of the human potential. Its advantage, conversely, lies in the logic of the self-fulfilling prophecy; out of tenacious hope, occasionally, human beings have indeed brought forth a partial realignment of history's currents.

As a historian, Thompson's influence will perhaps ultimately be measured in these terms: his ability to convince the present generation of historians to reassess the role played by popular movements and "popular culture" in the shaping of society. As far as his activism goes, it is still too soon to attempt any far-reaching judgment. On the one hand, Thompson's speeches during the 1980s seem (in retrospect) at times prophetic and at other times utopian; his vision of a peaceful and radically democratic Europe remains far from realization. On the other hand, he succeeded in meeting his own criteria for "partial free agency" in history; for, through his work in *END*, he prodded many Europeans toward a full-scale reappraisal of their place in world politics, and he did so through the power of persuasion, exercised at the grass roots.

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contemporary political developments on the Continent offered little evidence of any far-reaching "maturation."

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# The German and Catalan Peasant Revolts

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THIS ARTICLE COMPARES THE TWO LARGEST PEASANT UPRISINGS in late medieval and early modern Europe: the Catalan war of the *Remences*, which occurred in two phases from 1462 to 1486, and the German Peasants' War of 1524–1526. Even within a period of widespread social turbulence, these two conflicts stand out by reason of their geographical extent, duration, and scale. In Germany and Catalonia, as throughout late medieval Europe, rural disturbances and quarrels between tenants and landlords were frequent. Marc Bloch likened peasant agitation within the seigneurial economy of the Middle Ages to strikes in industrial capitalism—normal components of a system of production and exploitation.<sup>1</sup> There is, however, a difference between local riots (which were common) and large, widely diffused revolts (which were not).<sup>2</sup> The scope and the bitterness of two struggles in such different parts of Europe raise questions about peasant motivations and elite attitudes that cross physical and confessional boundaries.

The most obvious difference between the German and Catalan wars is the failure of the German and the success of the Catalan. While the German peasants were suppressed and never again mounted such an intense or broadly coordinated movement, the Catalan rebels achieved the official abolition of servitude in 1486.<sup>3</sup> The reasons for this disparity lie partly in the more favorable attitude of Catalan rulers toward peasant claims. Other factors involving the framing of demands and the response they evoked may also explain the contrasting outcomes. The comparison will show the degree to which it was possible to mount a massive assault on seigneurial institutions without a concomitant religious refor-

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<sup>1</sup> Marc Bloch, *French Rural History: An Essay on Its Basic Characteristics*, Janet Sondheimer, trans. (Berkeley, Calif., 1966; orig. publ. Paris, 1931), 170.

<sup>2</sup> A point made by Henry Kamen, "Bauernaufstände und dörfliche Gemeinde in Spanien und Europa im 16. und 17. Jahrhundert," in *Aufstände, Revolten, Prozesse: Beiträge zu bäuerlichen Widerstandsbewegungen im frühneuzeitlichen Europa*, Winfried Schulze, ed. (Stuttgart, 1983), 13–22.

<sup>3</sup> The German peasants were brutally suppressed, but some of their conditions were ameliorated; Thomas Sea, "The Impact of the German Peasants' War: The Question of Reparations," *Sixteenth Century Journal*, 8, no. 3 (1977): 75–97; Peter Blicke, *The Revolution of 1525: The German Peasants' War from a New Perspective*, Thomas A. Brady, Jr., and H. C. Erik Midelfort, trans. (Baltimore, Md., 1981; 2d German edn. publ. Munich, 1981), 165–69; Winfried Schulze, "Die veränderte Bedeutung sozialer Konflikte im 16. und 17. Jahrhundert," in *Der deutsche Bauernkrieg, 1524–1526*, Hans-Ulrich Wehler, ed. (Göttingen, 1975), 277–302.



mation. In Catalonia, as in Germany, a forceful moral justification lay behind the revolt, but it did not arise from a specifically anticlerical or evangelical sentiment.

The course of the German Peasants' War is better known and has provoked considerably greater debate.<sup>4</sup> The German war has been regarded as a crucial event in the overall history of the nation, and therefore its social context and consequences were, paradoxically, long subordinated to the perennial question of the origins of German disunity and consequent early modern backwardness. Until recently, the peasants' defeat was interpreted against the background of the victory of particularism and territorial principalities against the empire.<sup>5</sup> The events of 1524–1526 now tend to be seen in less grandiose terms, as an uprising whose agenda was not restoration of a unified Reich or an apocalyptic transformation but rather a response to secular economic conditions and the outgrowth of religious assumptions concerning the just ordering of society.

When historians have sought to place the German Peasants' War within a European setting, they have compared it to the English peasants' uprising of 1381 or the Hussite revolution in early fifteenth-century Bohemia.<sup>6</sup> These conflicts are especially useful in tracing the Christian radical inspiration for the German war. The combination of religious reform, anticlericalism, and a divinely sanctioned ideology of equality are evident in all three cases, although whether there was any direct causal connection from England or Bohemia to Germany is subject to doubt.<sup>7</sup>

Catalonia is a more cogent (if oblique) point of comparison. Catalonia presents a more sustained conflict than that of England and a more purely peasant revolt than that of Bohemia. It is closer chronologically to the German revolt than either the English or the Bohemian example, but most important, Catalonia shows what was ideologically possible within the context of traditional Catholic theories of justice (both popular and learned) and explanations for the divisions within

<sup>4</sup> On this extensive literature, see Tom Scott, "The Peasants' War: A Historiographical Review," *Historical Journal*, 22 (1979): 693–720, 953–74; H. C. Erik Midelfort, "The Revolution of 1525: Recent Studies of the Peasants' War," *Central European History*, 11 (1978): 189–206; Robert W. Scribner, "The German Peasants' War," in *Reformation Europe: A Guide to Research*, Stephen Ozment, ed. (St. Louis, Mo., 1982), 108–33; Tom Scott, "The Common People in the German Reformation," *Historical Journal*, 34 (1991): 183–92.

<sup>5</sup> In the words of Günther Franz, *Der deutsche Bauernkrieg*, 12th edn. (Darmstadt, 1984), 288: "Der Bauernkrieg ist ein Glied in dem Kampf der Deutschen um das Reich," a theme found even more strongly in the first (1933) edition. In addition to Franz, see Adolf Waas, *Die Bauern im Kampf um Gerechtigkeit, 1300–1525*, 2d edn. (Munich, 1976), 5–25. A critique of this approach is given by Horst Buszello, *Der deutsche Bauernkrieg von 1525 als politische Bewegung* (Berlin, 1969), 12–15; and by John C. Stalnaker, "Towards a Social Interpretation of the German Peasant War," in *The German Peasant War of 1525: New Viewpoints*, Robert W. Scribner and Gerhard Benecke, eds. and trans. (London, 1979), 23–28.

<sup>6</sup> M. M. Smirin, *Deutschland vor der Reformation: Abriss der Geschichte des politischen Kampfes in Deutschland vor der Reformation*, Johannes Nichtweiss, trans. (1952; Berlin, 1955), 102, 157; Frantisek Graus, "From Resistance to Revolt: The Late Medieval Peasant Wars in the Context of Social Crisis," in *The German Peasant War of 1525*, János M. Bak, ed. (London, 1976), 1–9; Horst Gerlach, *Der englische Bauernaufstand von 1381 und der deutsche Bauernkrieg: Ein Vergleich* (Meisenheim am Glan, 1969).

<sup>7</sup> Walter Müller, "Wurzeln und Bedeutung des grundsätzlichen Widerstandes gegen die Leibeigenschaft im Bauernkrieg 1525," *Schriften des Vereins für Geschichte des Bodensees und seiner Umgebung*, 93 (1975): 24–25.

society. Catalonia saw the eruption of a peasant war without a religious reform movement.

The Catalan war was both a political and social conflict. It arose from a struggle between the king on the one hand and an alliance of the city of Barcelona with a majority of the noble and clerical estates on the other.<sup>8</sup> Because of a longstanding constitutional crisis, the unpopular Trastámara dynasty had attempted to assert its authority against nobles and townspeople during a period of economic decline. The Trastámaras were Castilian and hence regarded as foreigners unconcerned about the condition of Catalonia, an impression bolstered by their ambitions in Naples and later Castile itself. A quarrel between King John II and his exceptionally popular eldest son, Charles, prince of Viana, resulted in the arrest of the prince, who died shortly thereafter (September 1461), precipitating a civil war between the king and the estates allied with Barcelona.

The ultimate cause of the constitutional confrontation, however, was not the internal conflict within the reigning family but rather the status of the enserfed peasants of northern Catalonia who were known as *Remences*.<sup>9</sup> Since 1388, the kings had tended to support the *Remences'* call for an end to their unfree condition. The peasants had organized to agitate for their emancipation in the decades preceding 1462. During the ten years of the first war, four foreign claimants to the throne were brought forward by the estates, while King John for his part asked the peasants for military support and also summoned the aid of King Louis XI of France (who soon switched sides). Despite the formidable array of opponents, John II outlasted and defeated his opposition by 1472, but his peasant allies did not immediately reap any benefit for their support. Another uprising in 1484 finally accomplished the abolition of servitude in return for substantial financial compensation paid by peasants to the lords.

Catalonia was smaller and more homogeneous than the areas of revolt that extended from one end of the German empire to the other. The German problems were more diverse, a series of individual conflicts stimulated by neighboring examples of revolt. I shall emphasize conditions in southwestern Germany (Swabia, the Upper Rhine, and the regions bordering Lake Constance), where, as in Catalonia, issues of serfdom came to the forefront of peasant grievances. The term "serf" is used here to denote unfree agriculturalists who, unlike slaves, formed legally recognized families settled permanently on particular tenements. They could not be bought and sold apart from the holding. They were considered chattels of their lords and disqualified from judicial and other public representation, but they acted within autonomous village communities.<sup>10</sup>

<sup>8</sup> Sources concerning the course of the war are published in the *Colección de documentos inéditos del Archivo de la Corona de Aragón*, vols. 14–26 (Barcelona, 1858–64). The major secondary works on the war and its immediate background are Jaume Vicens Vives, *Historia de los Remensas (en el siglo XV)* (1945; Barcelona, 1978); and Santiago Sobrequés i Vidal and Jaume Sobrequés i Callicó, *La guerra civil catalana del segle XV: Estudi sobre la crisi social i econòmica de la Baixa Edat Mitjana*, 2 vols. (Barcelona, 1973).

<sup>9</sup> On the development of this class, see Paul Freedman, *The Origins of Peasant Servitude in Medieval Catalonia* (Cambridge, 1991).

<sup>10</sup> In addition to the classic essays of Marc Bloch collected in *Slavery and Serfdom in the Middle Ages*, William R. Beer, trans. (Berkeley, Calif., 1975), see Pierre Bonnassie, "The Survival and Extinction of the Slave System in the Early Medieval West (Fourth to Eleventh Centuries)," 1–59; Bonnassie,

Medieval serfs were peasants. Like modern peasants, they held a specific parcel of land that they worked as a family and could pass on. They were constrained by extra-economic factors in such a way that their rent and personal bondage constituted a more coercive tie than one formed by a merely contractual relationship.<sup>11</sup> In the medieval period, many peasants (but by no means all) were serfs, but this legal status and its social significance (vestigial or constraining) varied with local conditions. In southwestern Germany, serfdom was used as a tool of territorial consolidation by princes and ecclesiastical lords, while in Catalonia it was essentially related to private landholding and exploitation.<sup>12</sup> By the mid-fifteenth century, however, in both regions, serfdom was widely resented by reason of its economic and symbolic import.

No pretense is made of a direct link between Catalonia and Germany. No one on the shores of Lake Constance or in the Black Forest was influenced by events in Catalonia or ever to my knowledge referred to the *Remences'* revolt. The very absence of such a connection makes it possible to posit, in common attitudes, a substratum of opinion within late medieval society that could lead to dramatic and revolutionary outbreaks. In examining these common elements, I am essentially concerned with exploring one of Barrington Moore's preconditions for peasant revolts: the point at which the exploitative character of the lord-tenant relationship becomes clear and insupportable.<sup>13</sup> When do people become angry and desperate enough to shift from evasion or quiet subversion to armed force? Anger and desperation aside, when are they confident enough not only of religious approval but of their own physical power to risk confrontation? These two revolts are not to be explained exclusively as responses to the general crisis of the late medieval economy. Other even more oppressed areas did not revolt (Germany east of the Elbe, for example); other revolts were more localized (France or Spanish Galicia). What explains the disillusion with usual methods of contesting the terms of subordination or the ability of these conflicts to transcend the localism of traditional societies?

Attitudes on the eve of these wars are particularly suggestive in light of the importance attributed to everyday forms of indirect resistance, such as passive

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"From One Servitude to Another: The Peasantry of the Frankish Kingdom at the Time of Hugh Capet and Robert the Pious (987-1031)," 288-313; Bonnassie, "Marc Bloch, Historian of Servitude: Reflections on the Concept of 'Servile Class,'" 314-40, all in Bonnassie's collection, *From Slavery to Feudalism in South-Western Europe*, Jean Birrell, trans. (Cambridge, 1991); and Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987), esp. 195-240.

<sup>11</sup> This definition of peasants relies on Teodor Shanin, introduction to his edited volume *Peasants and Peasant Societies: Selected Readings* (New York, 1971), 14-17; and Eric Wolf, *Peasants* (Englewood Cliffs, N.J., 1966), 1-17.

<sup>12</sup> Peter Blicke, "Leibherrschaft als Instrument der Territorialpolitik im Allgäu: Grundlagen der Landeshoheit der Klöster Kempten und Ottobeuren," in *Wege und Forschungen der Agrargeschichte: Festschrift zum 65. Geburtstag von Günther Franz*, Heinz Haushofer and Willi A. Boelcke, eds. (Frankfurt, 1967), 51-66; Vicens Vives, *Historia de los Remensas*, 11-36.

<sup>13</sup> Thus departing glaringly from what Moore terms the "folk conceptions of justice" and undermining the peasants' everyday forms of resistance; Barrington Moore, *The Social Origins of Dictatorship and Democracy* (Boston, 1966), 470-71. Moore's conditions for peasant revolt are mentioned in the context of the German Peasants' War by Jürgen Bücking, "Der 'Bauernkrieg' in den habsburgischen Ländern als sozialer Systemkonflikt, 1524-6," in *Der deutsche Bauernkrieg 1524-1526*, Wehler, ed., 169.

non-cooperation or sabotage.<sup>14</sup> Modern as well as earlier communities of peasants now tend to be viewed as capable of contesting and subverting the official requisites of their submission.<sup>15</sup> The explosions of the fifteenth and sixteenth centuries seem all the more unusual in the light of this revaluation of ordinary peasant resourcefulness. The great revolts indicate a failure of indirect opposition. The uprisings were a direct confrontation of authority, taken beyond the quotidian rituals of defiance and inversion. I would like to consider armed conflict as it might grow out of the established attitudes of German and Catalan peasants and their masters: to see the wars as the continuation of medieval resistance by other means.

A COMMON EXPLANATION FOR THE SCALE AND INTENSITY of the German Peasants' War is the revolutionary effect of the teachings of Martin Luther and other reformers such as Martin Bucer, Andreas Bodenstein von Karlstadt, or Huldrych Zwingli. The Reformation is thought to have galvanized the peasants' resentment, already prepared by the long habit of anticlericalism, and confirmed their sense of a right to resist authority.<sup>16</sup>

Most historians attribute the rebellion to a general climate of unrest not specifically focused on Luther. Günther Franz identified a religious impulse to social change, the invocation of a universally valid "Godly Law," as opposed to the circumscribed defense of custom or "Old Law."<sup>17</sup> Small local uprisings could take place when the community's sense of the prevailing rules and practices, the "good Old Law," was violated (as when lords encroached on common lands, raised formerly fixed dues, or imposed new exactions). By reason of the fragmented nature of German lordship, the defense of custom ("Old Law") would include at most a few villages at a time. According to Franz, a regional and supra-regional struggle on the order of 1525 required belief in a widely applicable divine law, a conviction that questioned all forms of seigneurial oppression and united peasants of diverse jurisdictions and village communities in a common effort.

It is evident that religious discontent and the Reformation had *something* to do with the revolt.<sup>18</sup> The problem comes in determining how new Godly Law really was in the sixteenth century and how distinct it was from Old Law justifications for revolt. The contrast is somewhat misleading. Old Law and divine law were not

<sup>14</sup> Described with reference to modern Malaysian peasants by James C. Scott, *Weapons of the Weak: Everyday Forms of Peasant Resistance* (New Haven, Conn., 1985). In more general terms, encompassing other subordinate groups, see James C. Scott, *Domination and the Arts of Resistance: Hidden Transcripts* (New Haven, 1990).

<sup>15</sup> For the Middle Ages, see, for example, R. B. Goheen, "Peasant Politics? Village Community and the Crown in Fifteenth-Century England," *AHR*, 96 (February 1991): 42–62.

<sup>16</sup> Henry J. Cohn, "Anti-Clericalism in the German Peasants' War of 1525," *Past and Present*, 83 (May 1979): 3–31; Heiko Oberman, "The Gospel of Social Unrest: 450 Years after the So-Called 'German Peasants' War' of 1525," *Harvard Theological Review*, 69 (1976): 103–29; Hans-Jürgen Goertz, *Pfaffenhass und gross Geschrei: Die reformatorischen Bewegungen in Deutschland, 1517–1529* (Munich, 1987).

<sup>17</sup> Franz, *Bauernkrieg*, 1–91.

<sup>18</sup> Emphasized, for example, by Oberman, "Gospel of Social Unrest"; Hans J. Hillerbrand, "The German Reformation and the Peasants' War," in *The Social History of the Reformation*, Laurence P. Buck and Jonathan W. Zophy, eds. (Columbus, Ohio, 1972), 106–36.

viewed by peasants as especially distinct. Both were used to oppose unjust positive law.<sup>19</sup> Local demands (such as rights to common land) that may now seem limited or petty were often phrased in terms of universal Christian law.<sup>20</sup> In Germany as elsewhere, custom and divine law coincided to legitimate a revolt against unjust exactions that violated God's ordinances.<sup>21</sup> In both Germany and Catalonia, attacks against serfdom were couched in these terms that combined antique practice and divine precept.

Criticism of peasant servitude long antedated the sixteenth century and did not necessarily require the radical teachings of the Reformation. As early as the thirteenth century, German serfdom was the object of attacks on the basis of Christian equality and the significance of Christ's sacrifice. These denunciations went beyond commonplace laments over human sinfulness to the point of asserting the illegitimacy of lordship that violated the teachings of biblical law.<sup>22</sup>

The centrality of serfdom in peasant unrest should be underscored. Although the revolt of 1525 involved a variety of complaints in many regions and principalities (war levies, other taxes, labor services, violation of fixed rents), the confrontation between landlords and peasants in southwest Germany turned on the intensification of serfdom. Peter Blickle's analysis of the grievance lists presented shortly before or during the outbreak finds that 90 percent include demands for the abolition of servitude.<sup>23</sup> Condemnation of servile status is found not only in southwest Germany but beyond: in the territory of the prince-archbishop of Salzburg, the diocese of Augsburg, Franconia, Alsace, and the Tyrol.<sup>24</sup>

Domination over tenants and their property marks the issue over which "social" and "religious" discontent came together. Demands for the end of servitude were the result of economic grievances inseparable from religious teachings. "Serf" was more than a mere word or archaic term. To be a serf meant exposure to a lord's arbitrary violence and consequently the absence of protections normally afforded by custom.<sup>25</sup> The pressure of seigneurial exactions levied by reason of servitude increased in fifteenth-century Germany, especially in the southwest.<sup>26</sup>

<sup>19</sup> See Heide Wunder, "'Old Law' and 'Divine Law' in the German Peasant War," in *German Peasant War*, Bak, ed., 54–62.

<sup>20</sup> Gerald Strauss, *Law, Resistance, and the State: The Opposition to Roman Law in Reformation Germany* (Princeton, N.J., 1986), 38–55, 106–20; Peter Bierbrauer, "Das Göttliche Recht und die naturrechtliche Tradition," in *Bauer, Reich und Reformation: Festschrift für Günther Franz zum 80. Geburtstag am 23. Mai 1982*, Peter Blickle, ed. (Stuttgart, 1982), 226–34.

<sup>21</sup> For England, see Rosamond Faith, "The 'Great Rumour' of 1377 and Peasant Ideology," in *The English Rising of 1381*, R. H. Hilton and T. H. Aston, eds. (Cambridge, 1984), 43–73. For France, Yves-Marie Bercé, "Offene Fragen der französischen Bauerrevolten vom 16.–18. Jahrhundert," in *Aufstände, Revolten, Prozesse*, 60–75; Bercé, *History of Peasant Revolts: The Social Origins of Rebellion in Early Modern France*, Amanda Whitmore, trans. (Ithaca, N.Y., 1990; orig. publ. Paris, 1986), 274–76.

<sup>22</sup> Bierbrauer, "Das Göttliche Recht," 217–26.

<sup>23</sup> Peter Blickle, "The Economic, Social and Political Background of the Twelve Articles of the Swabian Peasants of 1525," in *German Peasant War*, Bak, ed., 65.

<sup>24</sup> See Günther Franz, ed., *Quellen zur Geschichte des Bauernkrieges* (Darmstadt, 1963), 124–35, 154–56, 239, 305–09, 343; David Sabean, *Landbesitz und Gesellschaft am Vorabend des Bauernkriegs* (Stuttgart, 1972), 86–99; Franz Ludwig Bauman, ed., *Quellen zur Geschichte des deutschen Bauernkriegs in Oberschwaben* (1876; Hildesheim and New York, 1975), 63, 250, 310, 419, 509, 527; Hermann Wopfner, ed., *Quellen zur Geschichte des Bauernkriegs in Deutschirol 1525* (Innsbruck, 1908), 46, 61, 134–35.

<sup>25</sup> A point recently reemphasized by Bonnassie, "Marc Bloch, Historian of Servitude," 324–34.

<sup>26</sup> Blickle, *Revolution of 1525*, 68–86; Claudia Ulbrich, *Leibherrschaft am Oberrhein im Spätmittelalter*



In addition to its direct consequences, late medieval serfdom carried a tremendous baggage of humiliating imagery and stereotypes. A peculiarly dehumanizing discourse is found in both routine documents and in German satiric literature. A judicial proceeding over proof of unfree status in 1387, for example, provided an occasion for a witness to recall the harshness of the lords of Rocourt (in the bishopric of Basel, now the canton of Jura). Ferry, lord of Rocourt, responding to remonstrances of the mayor of the village, had said that the mayor knew very well that he was a serf and that, if Ferry wished, "he could take him by the foot and sell him in the market."<sup>27</sup> The same disturbing livestock metaphor appeared in a manifesto presented in 1525 by serfs of the abbey of Ochsenhausen (Upper Swabia), who stated they should not henceforth be subjugated, "to be sold like cattle and sheds, as we all have one lord, that is God in heaven."<sup>28</sup> Serfs belonging to the Teutonic Order's foundation at Beuggen (in the Upper Rhine region) complained, also in 1525, that they were imprisoned, chained by their necks for as long as two weeks at a time.<sup>29</sup> On the lands of the prince-archbishop of Salzburg, servile tenants complained that serfdom violated the law of the Gospels and that it amounted to a right to compel men to bend to seigneurial greed and brutality and be led around by the nose.<sup>30</sup>

The elites' images of peasants in the late Middle Ages tended to emphasize the subhuman nature of peasants and to commend savage treatment of them. German verse satires of peasant folly became conventional after the examples set by Neidhart von Reuenthal (a poet who died in 1236 or 1237).<sup>31</sup> Beginning in the fourteenth century, comic poems and plays in this tradition became both more grotesque and more violent. Neidhart had represented peasants as avaricious and stupid, perhaps as a humorous opposite to the values of the court. His protagonist is a poor knight victimized by boorish villagers.<sup>32</sup> In the Pseudo-Neidhart literature of the fourteenth and fifteenth centuries, however, his pathetic amorous attempts have been transformed into a series of sadistic pranks that demonstrate the bestial qualities of peasants and the virtues of tormenting or even

(Göttingen, 1979); Walter Müller, *Entwicklung und Spätformen der Leibeigenschaft am Beispiel der Heiratsbeschränkungen: Die Ehegenossame im alemannisch-schweizerischen Raum* (Sigmaringen, 1974).

<sup>27</sup> Joseph Trouillat, ed., *Monuments de l'histoire de l'ancien évêché de Bâle*, vol. 5 (Porrentruy, 1867), 886. Ferry also on at least two occasions claimed the right to compel tenants to spend the first night of their marriage where he should decide (Trouillat, 887–88). This is a degrading claim of lordship but not the infamous and largely legendary *droit de seigneur*. Compare the remonstrances of Catalan peasants, below, page 48.

<sup>28</sup> Buszello, *Der deutsche Bauernkrieg*, 17: "nit wie die kye und kölber verkouft werden, dieweil wir alle nur ein herren, das ist got den herrn im hymel, habe."

<sup>29</sup> Ulbrich, *Leibherrschaft*, 122.

<sup>30</sup> Günther Franz, ed., *Quellen zur Geschichte des Bauernkrieges*, 301: "Zu den 8. [that is, the eighth article of a grievance list] haben sich Geistlich und Weltlich frävenlich wider das Ewangeliumb, und haben sich des Eigentumb angezogen, das allain Got mit Aigentumb zuegehört, und die Menschen fur aigen under sich wellen biegen und schmuckhen und bei der Nesen in ir Geltnetz wellen ziehen."

<sup>31</sup> Fritz Martini, *Das Bauerntum im deutschen Schrifttum von den Anfängen bis zum 16. Jahrhundert* (Halle [Saale], 1944), 42–56; Eckehard Simon, *Neidhart von Reuenthal* (Boston, 1975).

<sup>32</sup> While Martini, *Das Bauerntum*, 42–56, sees the peasants in Neidhart's poetry as emblems of human folly in general, Erhard Jöst, "Bauernfeindlichkeit": *Die Historien des Ritters Neihart Fuchs* (Göppingen, 1976), 49–55, believes that Neidhart meant to attack peasants as a class, not as a symbol, and that his work was from the first read not as a comic dissent from courtly values, nor as a lament for human failings, but as a pointed denunciation of real peasants.

killing them.<sup>33</sup> Similarly, a fifteenth-century song, the "Edelmannslehre," instructs nobles to rob and pillage peasants and, if they have nothing, to wring their necks.<sup>34</sup>

However powerful the imputation of subhuman characteristics might have been, it ran up against the awkward fact that the peasants (free or servile) were Christian and therefore, in contemporary terms, fully human. Servitude represented a denial of human status, contradictory to Christian liberty. Possession of one person by another, or at least of one Christian by another, could not easily be reconciled with the Bible. Peasant rebels in 1525 demanded the abolition of serfdom not only on the basis of good old custom or natural law but because servitude was incompatible with Christ's incarnation and crucifixion. Christ died for all humanity, and the contradiction between this sacrifice and the subjugation of Christian peasants lay at the heart of remonstrances drawn up by peasants in 1525, in Alsace, Swabia, the region of Lake Constance, and in Salzburg.<sup>35</sup> This disparity was stressed by the most widely diffused list of demands, the "Twelve Articles of the Peasants of Swabia," drawn up in March of 1525 by Sebastian Lotzer, a lay preacher, on the basis of a multitude of earlier lists of peasant grievances.<sup>36</sup> Article Three begins: "It has hitherto been the custom for the lords to treat us as their serfs, which is pitiable since Christ has redeemed and bought us all by the shedding of his precious blood, the shepherd just as the highest, no one excepted."<sup>37</sup> The "Twelve Articles" served as the model for most subsequent programs presented by rebels during the war.

Sentiments in favor of peasants and lamenting their oppression are found in elite circles. Indeed, praise of rustic labor and complaints about its exploitation by other estates were something of a medieval commonplace. Even in German literary production, the encomium of the peasants (*Bauernlob*) was never completely obliterated by the more popular satire.<sup>38</sup> There was a persistent current of opinion that acknowledged the incompatibility of servitude with biblical teachings or Christian liberty. The familiar verse "When Adam delved and Eve span, who was then the gentleman?" (which also works as a rhyme in German) was repeated in urban and learned circles and had to be answered by apologists for servitude.<sup>39</sup>

The most significant connection between high culture and the demands of the

<sup>33</sup> *Die Historien des Neithart Fuchs nach dem Frankfurter Druck von 1566*, Erhard Jöst, ed. (Göppingen, 1980).

<sup>34</sup> Günther Franz, ed., *Quellen zur Geschichte des deutschen Bauernstandes im Mittelalter* (Darmstadt, 1974), no. 218, p. 553.

<sup>35</sup> Examples given in Müller, "Wurzeln und Bedeutung," 16, 18; Bierbrauer, "Das Göttliche Recht," 222–26; Franz, *Quellen zur Geschichte des Bauernkrieges*, 153, 169, 239, 301.

<sup>36</sup> Christoph Shappeler, a preacher in Memmingen, wrote the prologue and furnished biblical citations. Franz, *Quellen zur Geschichte des Bauernkrieges*, 174–79, trans. in Tom Scott and Bob Scribner, eds., *The German Peasants' War: A History in Documents* (Atlantic Highlands, N.J., 1991), 253–57; and in Blickle, *Revolution of 1525*, 195–201.

<sup>37</sup> Scott and Scribner, *German Peasants' War*, 254.

<sup>38</sup> For example, Hans Rosenplüt, a popular fifteenth-century writer of comic carnival plays that mocked rustics, was also the author of a poem in praise of the "noble peasant." Franz, *Quellen zur Geschichte des Bauernstandes*, 548–52, offers two contrasting excerpts from his work.

<sup>39</sup> On this theme and its diffusion, see Sylvia Resnikow, "The Cultural History of a Democratic Proverb," *Journal of English and Germanic Philology*, 36 (1937): 391–405; Martini, *Das Bauerntum*, 225–40; Hartmut Boockmann, "Zu den geistigen und religiösen Voraussetzungen des Bauernkrieges," in *Bauernkriegs-Studien*, Bernd Moeller, ed. (Gütersloh, 1975), 15.

German peasants was by means of the juridical works of the thirteenth century, especially the *Sachsenspiegel*, a legal treatise written in regional German.<sup>40</sup> Its author, Eike von Repgow, stated that God created man in His image and saved him with His suffering, the poor and the rich alike. Liberty was not simply a prelapsarian condition, lost because of human sinfulness (a standard explanation for lordship and inequality). Pope Gregory the Great, in words that were frequently repeated and incorporated into canon law, had stated that Christ's sacrifice redeemed all humanity from the consequences of original sin, breaking the bonds of slavery.<sup>41</sup> Eike von Repgow invoked this tradition and argued that there could be no legitimate servitude. It arose from the exercise of illegitimate power, not as a licit chastisement. Serfdom might be customary, but it was an abusive custom, made into law by prescriptive right but not to be confused with justice. The *Sachsenspiegel* also rejected excusing servitude on the basis of biblical divisions between privileged and condemned (Cain and Abel; Ham, Japheth, and Shem; Abraham and Ishmael; Jacob and Esau).<sup>42</sup>

More than Luther, Huss, or Wyclif, the contentious peasants of the early sixteenth century relied on ideas embodied in the *Sachsenspiegel* and other vernacular legal collections assembled centuries earlier. The "Twelve Articles" censured servitude in language close to that of the law books, as a violation of biblical injunctions and the salvation brought by Christ.<sup>43</sup> Thus, while the demands of the peasants of 1525 were encouraged and focused by the Reformation, these demands reflected an older tradition of moral opposition to serfdom. It was possible to contest oppressive practices and institutions by means of ideas that had force across the limits of class, region, and education. There is a fundamental opposition between the dehumanization of peasants, intended to separate them from the Christian (that is, human) community, and the defense of their humanity, from which follows the incompatibility of serfdom with the Gospels.

THAT A REVOLT AGAINST SERFDOM could base itself on pre-Reformation notions of justice is demonstrated most clearly by Catalonia. Of course, conditions were not the same as in Germany; therefore the comparison is just that, a comparison, not an exact match. Catalonia was not as politically fragmented as Germany. Its kings were weaker in the fifteenth century than previously, but there was a long earlier

<sup>40</sup> *Sachsenspiegel Landrecht*, Karl August Eckhardt, ed., Monumenta Germaniae Historica, Font. iur. Germ. ant. new ser. I, 1, 3d edn. (Göttingen, 1973), Book 3, p. 42. See Alexander Ignor, *Über das allgemeine Rechtsdenken Eikes Repgow* (Paderborn, 1984), 234–51; Bierbrauer, "Das Göttliche Recht," 222–27.

<sup>41</sup> Gregory I, *Registrum epistolarum*, Dag Norber, ed. (Corpus Christianorum, vol. 140) (Tournholt, 1982), 6: 12, p. 380: "Cum redemptor noster totius conditor creaturae ad hoc propitiatus humanam voluit carnem assumere, ut divinitatis suae gratia, disrupto, quo tenebamur capti vinculo servitutis, pristinae nos restitueret libertati." This is cited in the *Decretum* of Gratian, C. xii, Q. 2, c. 68.

<sup>42</sup> Discussed by Guido Kisch, *Sachsenspiegel and Bible* (1941; Notre Dame, Ind., 1990), 133–40. The arguments against serfdom in the *Sachsenspiegel* influenced later legal collections such as the *Schwabenspiegel* and the *Görlitzer Landrecht* (Ignor, *Über das allgemeine Rechtsdenken*, 237).

<sup>43</sup> See Müller's comparison table, "Wurzeln und Bedeutung," 29; and that of Bierbrauer, "Das Göttliche Recht," 226.

history of effective rule by the king and his parliament. As in Germany, a peasants' war was facilitated by the breakdown of political authority, in this instance defiance of the king by a parliament angry over the king's sympathy for the *Remences*. More than in Germany, a conflict over servitude was attached to a political struggle that divided the powerful.

In Catalonia, there existed the same subdivision of lordship and absence of lords from direct administration of their domains. Unlike Germany, however, there was no hint of an alliance between peasants and other subaltern groups such as urban artisans or miners. While a current of German historiography holds that the inspiration for revolt (as well as reform) came from the cities, no such claim can be made for Catalonia.<sup>44</sup> Finally, a longer, more steady building up of the conflict occurred in Catalonia. Germany experienced sharp but very brief revolts before 1525. In Catalonia, constant political agitation and frequent recourse to violence from about 1380 amount to what Pierre Vilar called "the Hundred Years' War of the Catalan countryside."<sup>45</sup>

As in Germany, Catalan peasants complained about serfdom in terms not only of its economic consequences but also of the implied indignity of the serfs and their helplessness before arbitrary power. On the eve of the revolt, in 1462, a grievance list presented by *Remences* mentioned the practices of forcing nursing mothers to serve as wet nurses at the lords' will and even the right supposedly claimed by lords to lie with a peasant bride on the night of her marriage.<sup>46</sup>

Above all, Catalan unfree peasants protested what were called "the bad customs," a term whose widespread use demonstrates the limits of euphemism in the Middle Ages.<sup>47</sup> Among the bad customs was the requirement of manumission in order to move away from a lord's jurisdiction (the origin of the Catalan word *Remença* is the Latin word for redemption). Other key exactions included certain levies on inheritance (especially when a peasant died without a direct heir) and on the property of a male peasant whose wife deserted him. Allied to the "bad customs" was a right of lords to "mistreat" their servile tenants, explicitly sanctioned by the Catalan estates at the opening of the thirteenth century. The Catalan *ius maletractandi* (right of mistreatment) resembled that claimed by northern lords such as the seigneur of Rocourt but was buttressed by more formal legal authority. As with the "bad customs," this term exemplifies the Catalans' explicit way of naming arbitrary power. The nature of this mistreatment included

<sup>44</sup> On different interpretations of the role of city and countryside in the German Reformation, see Bernd Moeller, *Imperial Cities and the Reformation: Three Essays*, H. C. Erik Midelfort and Mark U. Edwards, Jr., eds. and trans. (Durham, N.C., 1982); Tom Scott, *Freiburg and the Breisgau: Town-Country Relations in the Age of Reformation and Peasants' War* (Oxford, 1986), 190–235; Stephen Ozment, *The Reformation and the Cities: The Appeal of Protestantism to Sixteenth-Century Germany and Switzerland* (New Haven, Conn., 1975); Peter Blickle, "Social Protest and Reformation Theology," in *Religion, Politics and Social Protest: Three Studies on Early Modern Germany*, Kaspar von Greyerz, ed. (London, 1984), 4–15; Otthein Rammstadt, "Stadtunruhen in 1525," in *Der deutsche Bauernkrieg, 1524–1526*, 239–76.

<sup>45</sup> Pierre Vilar, *La Catalogne dans l'Espagne moderne*, vol. 1 (Paris, 1962), 379–80.

<sup>46</sup> The grievances of 1462 are edited in Eduardo de Hinojosa, *El régimen señorial y la cuestión agraria en Cataluña durante la Edad Media* (Madrid, 1905), appendix 11. The lords agreed to abolish compulsory wet-nursing and denied that any *droit de seigneur* had ever been claimed.

<sup>47</sup> What follows is dealt with in more detail in Freedman, *Origins of Peasant Servitude*, 106–53, 189–202.

confiscation of land and physical coercion, often a ritual humiliation involving stocks, tearing out of hair, and yoking together peasants like animals. Seigneurial violence beginning in the twelfth century created a vocabulary of oppressive gestures that would last until the end of serfdom in 1486.<sup>48</sup> The essential characteristic of a right of mistreatment (as opposed to ordinary jurisdictional power) was the ability to inflict harm on tenants without reason or need for justification.

Given the ability of lords to impose a harsh regime of tenancy, therefore, peasants in fifteenth-century Catalonia would hardly be expected to rely on an appeal to custom (at least one that encompassed recent human memory). Lords and peasants both retrospectively exaggerated the length of time that oppressive payments had been levied, believing them to have originated with Charlemagne. However evil the bad customs might be, they *were* customs, hallowed by time. Indeed, one line of moral defense offered by lords during the war was that Catalonia had flourished and become great under this agrarian regime; to change would threaten that greatness.<sup>49</sup> Attacks on the institutions of servitude were mounted by showing its incompatibility with divine law but also its contravention of basic assumptions of Catalan law, which was thought to guarantee the liberties of the Christian inhabitants of the principality—an Old Law argument but a *very* old law argument.

Much more than in Germany, Catalan arguments for and against serfdom were structured as competing myths of historical origins.<sup>50</sup> Legal commentators of the fourteenth and fifteenth centuries defended servitude, which they admitted was contrary to Catalan custom and to natural law, by referring its origins to a supposed failure of Christian peasants subject to the Muslims to heed the call of Charlemagne to rise up in support of his invading armies.

This is a historical-genetic myth of the origins of inequality, as opposed to the supranational biblical explanations that were popular in Germany. Servitude, according to this legend, was a merited punishment, no more disruptive of natural or divine law than putting criminals in jail, except that the castigation affected the heirs of the culprits. Serfdom was therefore explicable in terms of the particular history of the nation.<sup>51</sup> The Catalan myth of national origins was not sufficient to preserve beyond the fifteenth century the institution of serfdom it was intended to justify. As history, however, it was considered credible until the late nineteenth century.

<sup>48</sup> Thomas N. Bisson, "The Crisis of the Catalan Franchises (1150–1200)," in *La formació i expansió del feudalisme català*, Jaume Portella i Comas, ed. = *Estudi General: Revista del Col·legi Universitari de Girona*, vols. 5–6 (Girona, 1985–86), 153–72; Blanca Garí, "Las *querimonias* feudales en la documentación catalana del siglo XII (1131–1178)," *Medievalia*, 5 (1985): 7–49.

<sup>49</sup> As argued at the parliament meeting at Tortosa in 1429–1430, *Cortes de los antiguos reinos de Aragón y de Valencia y Principado de Cataluña*, vol. 16 (Madrid, 1912), 349–50.

<sup>50</sup> Paul Freedman, "Cowardice, Heroism and the Legendary Origins of Catalonia," *Past and Present*, 121 (November 1988): 6–10.

<sup>51</sup> A similar legend of national formation used as a historical defense of inequality appeared in late medieval and early modern Hungary. Historians and jurists believed that serfs were descended from those who failed to heed a call to the military assembly of the Huns (supposed ancestors of the Magyars). See Richard C. Hoffmann, "Outsiders by Birth and Blood: Racist Ideologies and Realities around the Periphery of Medieval Culture," *Studies in Medieval and Renaissance History*, n.s. 6 (1983): 14–20.



Catalonia demonstrated some of the same contempt for peasant character as Germany. The prolific Franciscan author of moral treatises, Francesc Eiximenis (died 1409), coupled sympathy for the urban poor with hatred of peasant malice and avarice, recommending that serfs be beaten, starved, and treated with what he called "terrible discipline" to forestall disobedience.<sup>52</sup> Nevertheless, Catalan elites during the late Middle Ages demonstrated considerable unease over how to reconcile Christian teachings with the subjugation of Christians. Jurists were not comfortable defending arbitrary seigneurial practices.<sup>53</sup> Some Catalan denunciations of servitude were as strident as those found in the German law books. A vivid statement was the opinion of a leading medieval legal scholar, Tomàs Mieres (ca. 1439) that neither the king nor parliament could legitimately promulgate a measure permitting seigneurial mistreatment, since such a law flagrantly violated divine precept.<sup>54</sup> This opinion was shared by members of the royal court, especially Queen Maria de Luna, who attempted to have her relative, the Avignonese pope Benedict XIII, declare the servile tenants of church lands free. In her letter of 1402, Queen Maria deployed adjectives such as "execrable," "abominable," and "pestiferous" to characterize servitude and also stated flatly that it was against God and justice.<sup>55</sup>

Most significant is what survives of arguments made by peasants and their apologists in the years leading up to the outbreak of violence. There is less of this material in Catalonia than in Germany, largely because the printing of complaints and calls to action in the form of semi-clandestine pamphlets (*Flugschriften*) was possible in 1525 but not in the mid-fifteenth century.

The most extensive defense of the *Remences'* agitation is the prologue to a record of oaths sworn by the inhabitants of several hundred peasant villages in 1448–1449.<sup>56</sup> The prologue justified the formation of syndicates to solicit the abolition of serfdom by appealing to Christian justice framed as a response to the seigneurial myth of the origins of serfdom. It begins by citing the above-mentioned passage from Gregory the Great, asserting the fundamental equality of all Christians for whom Christ's sacrifice was accomplished without distinction. Christ "broke the bonds of servitude by which humans were made captive and restored us to our original liberty."<sup>57</sup> In an ingenious turn to the conventional

<sup>52</sup> Francesc Eiximenis, *Lo Crestia*, Book 12, cited in Jill Webster, ed., *Francesc Eiximenis, la societat catalana al segle XIV* (Barcelona, 1967), 59.

<sup>53</sup> The thirteenth-century standard gloss to the *Usatges of Barcelona* acknowledged that the right to mistreat tenants was contrary to Roman and Visigothic law; *Antiquiores Barchinonensium Leges* (Barcelona, 1544), fol. 109v. In the fourteenth century, Jaume de Montjuich stated that the laws permitting mistreatment were in accord with human rather than divine justice; *Antiquiores*, fol. 37v. Divergent opinions on whether the *ius maletractandi* could be considered licit are preserved in two fifteenth-century manuscripts, *La Seu d'Urgell*, Arxiu de la Catedral, MS 2108, fols. 134r–136v; Barcelona, Biblioteca de Catalunya, MS 485, fol. 58v.

<sup>54</sup> Tomàs Mieres, *Apparatus super constitutionibus curiarum generalium Cathaloniae*, vol. 2 (Barcelona, 1621), 513.

<sup>55</sup> Francisco Monsalvatje y Fossas, ed., *Noticias históricas*, vol. 13 (*Historia de la guerra social de los Remensas*) (Olot, 1906), documents no. 1,738 and 1,740.

<sup>56</sup> Girona, Arxiu Històric de l'Ajuntament, Secció XXV.2, Llibres manuscrits de tema divers, lligall 1, MS 8, fols. 1r–2v, Freedman, ed., *Origins of Peasant Servitude*, 224–26.

<sup>57</sup> Compare the texts excerpted by Bierbrauer, "Das Göttliche Recht," 226, the *Schwabenspiegel* (from 1280): "God created men in his own image and also by his agony redeemed them from death";

myth of peasant betrayal and consequent serfdom, the ancestors of the *Remences* were not Christians laboring under Muslim oppression: rather, they were Muslims! The Frankish conquerors naturally brought pressure to convert them to Christianity. Many inhabitants did accept baptism then and there, but some were stubborn and so were condemned to labor in unchristian servitude, until they should come to see the error of what the document calls their "paganism." The "bad customs" and all the attendant paraphernalia of servitude were justly applied to non-Christians but only to stimulate conversion. As these practices were manifestly contrary to the teachings of the true religion, they were never supposed to burden Christians. In 1448, there were, of course, no more Muslim peasants. The only reason the oppressive customs had survived, therefore, was because of the greed and violence of the lords.

Thus Catalan explanations for the origins of injustice against peasants juxtaposed Christian conduct with unjust social reality but differed from the German case by refraining from demanding even the limited transformation of society that the Reformation required. The wealth and secular power of monasteries, for example, which figure prominently in Erasmus's criticisms, in Luther's tracts, and in the German peasant grievances, were never singled out in Catalonia. There was no call to elect pastors locally, to destroy clerical power, or to assert the teachings of the Gospel against ecclesiastical practice. Such desires, fundamental to peasant agitation in England, Bohemia, and Germany, were absent in Catalonia.

If there was no religious reformation, what then explains what was, after all, the longest and most successful peasant war of the late medieval or early modern period? A Christian egalitarian doctrine is discernible, but its roots were deeper than the reform currents of the fifteenth or sixteenth centuries. Ideas of social justice already existed within the conventional array of medieval intellectual possibilities, which formed the basis for both the reluctance of jurists and theologians to justify serfdom and for the arguments of peasants and their defenders. The Catalan war was the product of a harsh but unevenly applied seigneurial regime whose exactions were never perceived wholeheartedly as compatible either with Catalan traditions or Christian liberty. That the peasants took advantage of a constitutional crisis cannot be questioned, but the crisis was itself the result of peasant unrest and—at least intermittent—royal encouragement.

I RETURN TO BARRINGTON MOORE'S PRECONDITIONS for revolt, in particular the point at which the exploitative nature of a seigneurial relationship becomes manifest. Was there actually a sudden moment of disillusion with an economy previously regarded as thoroughly legitimate? Medieval historians at one time tended to idealize the mutuality of service and protection between peasant and lord. The feudal relationship was thought of as a reasonable exchange of

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and the *Twelve Articles* (1525): "Christ redeemed and bought us all in shedding his most precious blood, the shepherd just as much as the highest, with none excepted."

agricultural work and loyalty in return for defense.<sup>58</sup> This organic model of medieval social order has proven surprisingly durable, especially among those who attempt to establish a starting point for disintegration, before which (1500? 1350?) social cohesion held and hierarchy was unquestioned.<sup>59</sup> In fact, it is doubtful that there was a time when peasants fatalistically accepted the legitimacy (as opposed to sheer power) of exploitation.<sup>60</sup> The effective but oppressive nature of the seigneurie has been shown in the work carried out by the French school with its emphasis on the economically productive but socially brutal "feudal revolution" of the eleventh century. Moreover, the extent of peasant discontent has been underscored by influential historians of early modern rural mentalité who portray members of lower orders as purposeful social actors, not fixed in a frozen mental world or unable to perceive or desire change.<sup>61</sup>

It is not necessary, therefore, to search for a single event or cause of peasant disillusionment with the seigneurial regime. Rebellion did not require a dramatic external stimulus such as millenarian expectation or imported urban concepts of equality. It was possible to have a sustained, large-scale peasant war without benefit of a radical reform or anticlerical sentiment. In Catalonia, basic notions of justice coexisted with rather moderate demands, realized, however, by radical means of implementation. As in most of Germany, the Catalan peasants accepted the prevalence of lordship and most of its perquisites. They sought the dissolution of servitude and arbitrary exploitation but not of property holding or even privilege. They were inspired by religious ideas but applied traditional interpretations of biblical equality to the defense of local autonomy.

The Catalan peasant war discloses certain limitations in historiographic typologies of peasant resistance and ideology. Even the updated version of the Old Law–Godly Law distinction is difficult to apply. In Catalonia, it was the lords who defended custom; the peasants' moral argument combined divine purpose and historical legitimation. In Germany, there was a greater sense of the recent imposition of abusive lordship, and so the defense of local custom accompanied appeals to the Gospel. Even so, in Germany, resistance did not require the sanction of Reformation ideas, although the religious upheaval created a political situation similar to (if ultimately less favorable than) that afforded by Catalonia's constitutional paralysis.

The distinction advanced by Peter Burke between traditionalist justifications for revolt and radical visions of a new order is not readily applicable to the German or Catalan wars in which a radical means (a large-scale military uprising)

<sup>58</sup> As presented, for example, by Otto Brunner, *Land und Herrschaft: Grundfragen der territorialen Verfassungsgeschichte Österreichs im Mittelalter*, 5th edn. (Vienna, 1965).

<sup>59</sup> On the naïve but persistent notion that medieval society was static and hierarchical, see Lee Patterson, "On the Margin: Postmodernism, Ironic History, and Medieval Studies," *Speculum*, 65 (January 1990): 94–100.

<sup>60</sup> James Scott argues effectively against the assumption that false consciousness is imposed hegemonically by seigneurial elites; *Domination and the Arts of Resistance*, 70–107.

<sup>61</sup> Emmanuel Le Roy Ladurie, *The French Peasantry 1450–1660* (1977; Aldershot, 1987), 359–99; Carlo Ginzburg, *The Cheese and the Worms: The Cosmos of a Sixteenth-Century Miller*, John Tedeschi and Anne Tedeschi, trans. (1976; Baltimore, Md., 1980); Ginzburg, *Night Battles*, Tedeschi and Tedeschi, trans. (1966; Baltimore, 1983); Natalie Z. Davis, "The Rites of Violence: Religious Riot in Sixteenth-Century France," *Past and Present*, 59 (May 1973): 51–91, reprinted in Davis, *Society and Culture in Early Modern France* (Palo Alto, Calif., 1975), 152–87.

resulted from attempts at a limited reordering of society along what were conceived as traditional lines.<sup>62</sup>

Typologies of peasant discontent are useful, if arbitrary, since the varieties of scale and motivations of rural attitudes and uprisings must be taken into account. Yet typologies are flawed by the desire to posit "real" or "serious" movements for change as against "traditional" or conservative ones. This is a vestige of the view of peasants as essentially passive, or as passive with occasional despairing but futile spasms of rage.

It is not to dispute the uniqueness and importance of both the Catalan and German peasant wars to assert that they grew out of certain longstanding climates of dissent rather than being entirely created by sudden external events. By allowing comparison across the divide erected by the Reformation, the Catalan war throws into better relief certain aspects of the German conflict. Catalonia's experience of a major peasant war without a radical religious reform movement calls into question the belief that the German Peasants' War must have had an origin unrelated to conditions in the countryside.

In both Catalonia and southwestern Germany, the key conflict was over servitude. The issue is not so much peasant ideology as an immutable thing in itself as it is a particular, focused hatred of serfdom. The two regions shared the intensification of a harsh personal bondage and the resistance of peasants organized in local communities that had not succumbed to a regime of large estates. Peasant beliefs derived from biblical ideas of equality, and what turned such attitudes into action was not so much an external event (the Reformation) as resentment against an intensified servitude and its attendant indignities.

What, then, explains the success of the Catalan *Remences* and the failure of the German movement? The breakdown of governing authority in Catalonia was more complete, and peasants there had an even longer history of active resistance than did those in Germany. The key difference, however, appears to be the greater doubt in elite political and intellectual circles in Catalonia over justifying servitude. In Germany, too, to be sure, there was a tradition of social criticism reaching back, as we have seen, to the thirteenth-century law books, reflected also in the *Reformatio Sigismundi* of 1439 and the tract written by the so-called Upper Rhine Revolutionary (ca. 1490).<sup>63</sup> But in Catalonia, even jurists who defended serfdom acknowledged its discrepancy from natural law and normal Catalan custom.<sup>64</sup> The royal court, though wavering and opportunistic, supported the demands of the peasants. There was a broader dissent in Catalonia than in Germany over the question of serfdom. Arguments were less likely in Catalonia than in Germany to center on biblical texts (such as Noah's curse on the

<sup>62</sup> Peter Burke, *Popular Culture in Early Modern Europe* (New York, 1978), 173–78.

<sup>63</sup> *Reformation Kaiser Sigismunds*, Heinrich Koller, ed., *Monumenta Germaniae Historica, Staatschriften des späten Mittelalters*, vol. 6 (Stuttgart, 1964), esp. 84, 92, 168, 276–80; *Das Buch der hundert Kapitel und der vierzig Statuten des sogenannten Oberrheinischen Revolutionärs*, Annelore Franke and Gerhard Zschäbitz, eds. (Berlin, 1967). See the discussion by Hartmut Boockmann, "Zu den geistigen und religiösen Voraussetzungen des Bauernkriegs," 11–15. The author has been identified as the imperial secretary Mathias Wurm by Klaus H. Lauterbach, "Der 'Oberrheinische Revolutionär' und Mathias Wurm von Geudertheim," *Deutsches Archiv*, 45 (1989): 109–72.

<sup>64</sup> Paul Freedman, "Catalan Lawyers and Servitude," *Mediaeval Studies*, 48 (1986): 294–304. See also above, note 53.

descendants of Ham) than on the historical or mytho-historical constitution of the nation. It was widely and openly conceded that serfdom violated divine precept. A second line of defense, that of noble heroism versus peasant cowardice, did not have the moral power to withstand peasant agitation and rebellion.

There were fissures in what at first glance appear to have been hegemonies of social certainty. Not only was the elite theater of domination—its rituals of authority—less than wholly accepted by those it purported to convince but the gestures could at certain times provoke considerable dissent among the performers themselves. Peasants and their spokesmen deployed a rhetoric of moral power drawn from an arsenal of Christian teaching preached to elite and humble alike. It is not merely that peasants might draw more radical conclusions from this essentially shared discourse<sup>65</sup> or that they misread teachings of Christian liberty.<sup>66</sup> Rather, they perceived the points of weakness or uncertainty within the structure of political and economic power that faced them. One can pity the apparent naïveté of the Stühlingen peasants' plaintive statement (Article 23 of their list of grievances) that "we do not know the origin of rents and interest payments" or their asking for a "credible" explanation for them.<sup>67</sup> This request cannot be said to have met a reasoned or compassionate response. When moral demands were placed, however, in terms not only of Christian liberty but national tradition as well, as in Catalonia, their force was considerable and commanded a reluctant acquiescence.

In a treatise on the vices of different estates written around 1500 in France, the anonymous author begins with Matthew 19:24, in which Christ likens the difficulty of the rich man entering heaven to that of a camel passing through the eye of a needle. His book, the author tells us, was inspired by meditation on these "very hard and weighty words" of the Gospel.<sup>68</sup> It is easy to dismiss such thoughts as sociologically irrelevant and at most historical moments they probably are. Yet in the Germany of 1525, and in Catalonia of 1462, the anger of peasants and the doubts of the elite over serfdom contributed to the two largest uprisings in pre-modern Europe.

<sup>65</sup> Argued by Stephen J. Greenblatt, "Murdering Peasants: Status, Genre, and the Representation of Rebellion," *Representations*, 1 (1983): 21; and J. Scott, *Domination and the Arts of Resistance*, 79.

<sup>66</sup> Winfried Becker, "'Göttliches Wort,' 'göttliches Recht,' 'göttliche Gerechtigkeit': Die Politisierung theologischer Begriffe?" in *Revolte und Revolution in Europa: Referate und Protokolle des International Symposiums zur Erinnerung an den Bauernkrieg 1525*, Peter Blickle, ed. (Munich, 1975), 232–63.

<sup>67</sup> Scott and Scribner, *German Peasants' War*, 68.

<sup>68</sup> Paris, Bibliothèque Nationale, MS Français 1148, fols. 1<sup>r</sup>–1<sup>v</sup>.



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# The Peasant Woman in Stalinist Political Art of the 1930s

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VICTORIA E. BONNELL

THE PEASANT WOMAN PRESENTED THE MOST COMPLEX AND CONTROVERSIAL image in the lexicon of Soviet political art.<sup>1</sup> In contrast to the male peasant, whose figure graced some of the earliest Bolshevik political posters of 1918,<sup>2</sup> a standardized representation of the female peasant (*krest'ianka*) did not appear until May Day 1920. Nikolai Kochergin's memorable poster produced for that occasion depicted her as a buxom woman wearing a kerchief and bast shoes, holding a sickle, and striding forward, alongside a male peasant and male worker, on the debris of the old order (Figure 1).<sup>3</sup> Kochergin's image, which drew heavily on prerevolutionary sources,<sup>4</sup> was often reproduced in visual propaganda of the 1920s.

Although women peasants belonged nominally to one of the four social groups (together with male and female workers and male peasants) that were officially accorded heroic status in the revolution, their image in political art conveyed a contradictory message. As depicted from 1920 on, the peasant woman conjured up associations with the *baba*, a term used to describe a peasant woman whose traits, depending on context, included fecundity and shrewdness as well as ignorance, greed, and subordination to the patriarchal rural world.<sup>5</sup> The visual

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<sup>1</sup> Research for this essay has been based primarily on an examination of about 6,500 political posters at the Hoover Institution Archives and the Poster Collection at the Lenin Library (now the Russian State Library), as well as numerous posters in contemporary journals devoted to visual propaganda. Of these, I have created a database that includes detailed information on 996 posters produced in Soviet Russia from 1918 to 1953. My research also included visual displays at holiday celebrations and monumental sculpture.

<sup>2</sup> Aleksandr Apsit's November 1918 poster, "Year of the Proletarian Dictatorship, October 1917–October 1918" ("God proletarskoi diktatury, Oktiabr' 1917–oktiabr' 1918"), exemplifies the new image.

<sup>3</sup> References to posters located in the Russian and Soviet poster collection at the Hoover Institution Archives, Stanford University, are henceforth designated with the prefix RU/SU. Kochergin's poster is reprinted in Stephen White, *The Bolshevik Poster* (New Haven, Conn., 1988), 62, and can be viewed at the Hoover Archives, RU/SU 2087. The following year, the artist Ivan Simakov produced a May Day poster incorporating the same image of the female peasant; RU/SU 1373. The image reappears in many other posters during the 1920s.

<sup>4</sup> See, for example, "Shel avstriets v Radzivilly," produced between 1914 and 1917 by political artists working in the *lubok*, or Russian folk art tradition; RU/SU 61A. The poster is reproduced in V. Denisov, *Voina i lubok* (Petrograd, 1916), 36.

<sup>5</sup> For a discussion of the complex associations with the images of the peasant woman, see my essay, "The Representation of Women in Early Soviet Political Art," *Russian Review*, 50 (July 1991): 267–88.



FIGURE 1: A poster by Nikolai Kochergin, "Ioe maia" (The First of May) (1920), courtesy of the Hoover Institution Archives, Stanford University, Stanford, California.

syntax, that is, the relationship among images, further accentuated the ambiguous position of the *krest'ianka*. Before 1930, she seldom, if ever, appeared alone in political posters but nearly always in a relationship of contiguity to other, more positive social categories. By this device, Soviet artists indicated to viewers that

A contemporary poster by Mikhail Cheremnykh, "Istoriia pro bubliki i pro babu" (Story of the Bagels and the Baba), produced in 1920, exemplifies the negative representation of the woman peasant.

women in the countryside occupied a lesser place in the Bolshevik hierarchy than workers (unambiguously heroic) or male peasants and acquired heroic status only in their presence.

The stout woman with a sickle continued to serve as the symbolic representation of the female peasant until the onset of collectivization, when a dramatic shift took place in visual propaganda, and gender designations acquired new significance. Soviet artists represented collectivization in the female idiom, placing the image of the rural woman—now the collective farm worker, or *kolkhoznitsa*—in the foreground of many posters. The *krest'ianka* with the sickle was replaced by the *kolkhoznitsa* on a tractor—a new figure with distinct attributes. The imagery was novel but so was the syntax. In this new visual language, the definition of the female peasant no longer depended on a relationship of contiguity to others in the Bolshevik pantheon. She sometimes stood alone or in front of the male peasant; she was occasionally even portrayed in the larger-than-life format formerly reserved for heroic male workers and soldiers.

The purpose of this essay is to examine these important changes in Stalinist political iconography<sup>6</sup> during the early 1930s as well as subsequent transformations during the Second Five-Year Plan. The “great retreat” in policy that began in 1934 had its visual counterpart, introducing important modifications in the grammar and lexicon of political art. My aim, above all, is to analyze the Stalinist discourse on power as it was conveyed through the visual medium.

BOLSHEVIK POLITICAL ART first took shape during the Civil War, when more than 450 groups turned out thousands of posters to mobilize and propagandize the population. Following the adoption of the New Economic Policy in 1921, poster production slackened considerably and lost a good deal of its earlier fervor, intensity, and artistic originality. A massive propaganda effort resumed in 1929 at the inception of the First Five-Year Plan. Political posters provided a major means for reaching a society that was only partially literate and had a strong visual traditional culture.

In 1929 and 1930, a multiplicity of organizations feverishly turned out tens of thousands of posters promoting collectivization. No figures are available on the total number of posters relating to collectivization, but one publisher—the State Publishing House for Visual Arts (Izogiz)—put out 21 posters on this theme during the first three months of 1930, with a total of 600,000 posters.<sup>7</sup> The total for the USSR was far higher. In the first quarter of 1931, Izogiz produced 125

<sup>6</sup> The term iconography is used here to refer to standardized images in Bolshevik political art that depicted heroes (saints) and enemies (the devil and his accomplices) according to a fixed pattern (the *podlinnik* in Russian Orthodox church art). The icons were part of a system of signs imposed by the authorities in an effort to transform mass consciousness. Iconographic images were consistent and incessantly repeated, and they resonated strongly with mythologies from the Russian past.

<sup>7</sup> *Brigada khudozhnikov*, nos. 2–3 (1931): 22. According to an article in *Za proletarskoe iskusstvo*, no. 1 (1931): 29, Izogiz produced 17 posters for the spring sowing campaign in 1930. The entire output of posters by Izogiz in the first quarter of 1930 (all themes combined) was 62.

posters, more than twice as many as during the preceding year. Many of these promoted the theme of collectivization.<sup>8</sup>

Political art did not come under one central authority until March 11, 1931, when the Central Committee of the Communist Party issued a resolution emphasizing the importance of political posters as a "powerful tool in the reconstruction of the individual, his ideology, his way of life, his economic activity" and a means of "entering the consciousness and hearts of millions [of people]."<sup>9</sup> The pronouncement set forth the ambitious task of political art in the Stalin era: to change people's structure of thinking at its deepest subrational level, where critical faculties could not be fully exercised.

In accordance with the enhanced role of visual propaganda, all poster production was placed under the authority of Izogiz. Having previously functioned as only one of a number of publishing houses engaged in the production of political posters, Izogiz now became the sole publisher of posters, operating under the direct supervision of the Central Committee. The centralization of poster production ensured uniformity in the kinds of images and messages conveyed by visual propaganda. Even before the resolution went into effect, certain distinctive trends could be discerned in political art. With the advent of the First Five-Year Plan, Soviet poster artists searched for new forms of expression and new ways of depicting the heroes of socialist construction.

In the course of 1929, a novel image of the peasant woman appeared in Soviet Russia: the *kolkhoznitsa*. She was featured in Sergei Eisenstein's film *Novyi i staryi* (Old and New, or The General Line, as it was originally titled), released in October 1929.<sup>10</sup> The centerpiece of the film is a determined young peasant woman who helps to establish a collective farm, or *kolkhoz*; the villagers resisting collectivization ridicule her efforts and label her a *baba*. After much difficulty, she obtains a tractor for the farm. In the final scene of the film as originally edited by Eisenstein, she is pictured triumphantly at the wheel of the tractor.

A similar recasting of the female peasant image was taking place in political posters. In 1929, a poster by I. Meshcheriakova, "To Collective Work" ("Na kollektivnuiu rabotu"), anticipated visual conventions that became standard in the 1930s (Figure 2).<sup>11</sup> It shows a group of peasants (men and women) cheerfully going to work in the fields. Women are prominently featured in the center of the poster. A woman is shown driving a tractor (a male tractor driver precedes her).

Peasant women of any description appeared rather seldom in posters produced between 1918 and 1929. But a change took place in 1930, just as the collectivization campaign was gathering momentum. Beginning in that year, the peasant woman, now transformed into a *kolkhoznitsa*, is frequently included in political art with a rural theme. In my research on propaganda posters produced in the early 1930s, I was struck by the frequency of female images. A quantification of the evidence shows that images of peasant women did occur often in collectivization

<sup>8</sup> *Za proletarskoe iskusstvo*, no. 1 (1931): 29.

<sup>9</sup> *Brigada khudozhnikov*, nos. 2-3 (1931): 1-3.

<sup>10</sup> The original version of the film can be viewed at the Pacific Film Archive, Berkeley, California. For a discussion of the history of the film, see Jay Leyda, *Kino: A History of the Russian and Soviet Film* (London, 1960), 262-69.

<sup>11</sup> RU/SU 1658.





FIGURE 2: A poster by I. Meshcheriakova, "Na kollektivnuiu rabotu" (To Collective Work) (1929), courtesy of the Hoover Archives.

posters. Out of a sample of 175 political posters on the theme of agriculture issued between 1930 and 1934, I have found that 106 posters (61 percent) included images of women. Women occupied a central or prominent place in 68 posters or 39 percent of the total.<sup>12</sup>

The volume of poster production does not tell the whole story; the size of the printing also gives an indication of the importance of certain posters. A typical press run in the early 1930s ranged from 10,000 to 30,000. But some posters were produced in enormous volume, with as many as 100,000 copies. Posters deemed especially important appeared in special editions in the languages of various groups within the non-Russian national republics.

<sup>12</sup> Posters for this sample were examined at the Hoover Archives and the Poster Collection at the Lenin Library. To my knowledge, there is no published or unpublished catalogue of posters produced in this period. My sample includes all the posters on the theme of agriculture that were issued between 1930 and 1934 or can be reasonably attributed to that period. I have excluded multiple editions of posters in various national languages. In defining posters in which women occupied a central or prominent place, I used the following guidelines: 1) posters exclusively featuring women (one or more); 2) posters with women and men, in which women are not depicted as subordinate to men by virtue of their activity or placement in relation to men; 3) any poster in which a woman is shown driving a tractor. I examined all posters relating to collectivization in the Hoover Archives (total collection houses about 3,000 Russian and Soviet posters). At the Lenin Library collection (containing some 400,000 posters), I had no access to the catalogue. I therefore requested all posters pertaining to the collectivization campaign.



Some posters featuring female peasants were issued in very large numbers. For example, in 1930, the poster by Z. Pichurgin, "Collective Farm at Work" ("Kolkhoz v rabote"), appeared in a printing of 100,000. In the same style as the Meshcheriakova poster of 1929, Pichurgin placed two sturdy young peasant women in the center of the composition. They are gathering hay. To their left, the young male driver of a horse-drawn harvester smiles at them as he passes by.<sup>13</sup>

Details of this idyllic harvest scene illustrate the new visual language that was becoming established. Young, trim women are shown in the act of working; the old class marker, the sickle, has disappeared (the tractor will take its place). Each woman wears a red kerchief tied behind her head, in the style of women workers, rather than under the chin, as was formerly conventional in the representation of peasant women. Details of appearance, such as the style of the kerchief, conveyed a message to viewers that the *kolkhoznitsa* was different from the *baba* of the past; she belonged to a new breed of *homo sovieticus* in the countryside.

The new imagery was epitomized by Vera Korableva's widely disseminated and memorable poster, "Come, Comrade, Join Us in the Collective Farm!" ("Idi, tovarishch, k nam v kolkhoz!") (Figure 3). The poster was first issued in 1930 and then reissued in 1931 and reproduced in many different national languages. The 1931 Russian-language version alone consisted of 40,000 copies, but the total output far exceeded that number.<sup>14</sup> It shows a young woman standing in front of the young male peasant tractor driver; she is calling to others to enter the *kolkhoz*. Her companion is smiling, and she, too, has a cheerful look about her. She is placed in the dominant position in the poster (in front of the man and engaged in action); her appearance replicates key features of the Pichurgin peasant woman discussed above.

Korableva was only one of a number of talented political artists who created memorable posters on the theme of collectivization. "Go to the Collective Farm" ("Idi v kolkhoz") by Nikolai Terpsikhov (Figure 4), appeared in a printing of 100,000 and in many different languages, including Ukrainian.<sup>15</sup> The most important poster in the regime's campaign to halt the massive slaughter of livestock by forcibly collectivized peasants, it shows a young peasant woman leading a horse and cow into the collective farm. She looks directly at the viewer with a penetrating gaze.<sup>16</sup> Here again, the artist has incorporated the new features of the *kolkhoznitsa*.

Terpsikhov's poster illustrates another major development in the visual language used to depict rural life in the early 1930s. Whereas formerly, peasant women were defined by their place in a hierarchy dominated by the image of the urban worker, now they began to appear alone in posters. In other cases, they were placed in front of and in a prominent position vis-à-vis male peasants (as in

<sup>13</sup> RU/SU 1655.

<sup>14</sup> A reproduction of the poster can be found in *Sovetskii politicheskii plakat/The Soviet Political Poster* (Moscow, 1984), illus. 58. The original version of the poster, together with versions issued in various national languages, can be found in the Poster Collection at the Lenin Library in Moscow, henceforth designated by the prefix KP (Kolleksiia plakatov). See KP p4.ix.3a idi. It is also available in the Hoover Archives, RU/SU 641.

<sup>15</sup> RU/SU 1856. The Ukrainian version was issued in a printing of 10,000. For this and other versions in various languages, see KP p4.ix.31 ide.

<sup>16</sup> RU/SU 1856.



FIGURE 3: A poster by Vera Korableva, "Idi, tovarischch, k nam v kolkhoz!" (Come, Comrade, Join Us in the Collective Farm!) (1930), courtesy of the Hoover Archives.

the Korableva poster, Figure 3). These kinds of compositions were virtually unprecedented in Soviet political art before 1930. To be sure, three out of five posters dealing with collectivization continued to represent peasant women in a secondary or recessed position in relation to men. But some posters contained a new visual syntax for placing the *kolkhoznitsa* in the hierarchy of heroic groups.



FIGURE 4: A poster to halt the slaughter of livestock, by Nikolai Terpsikhov, "Idi v kolkhoz" (Go to the Collective Farm) (1930), courtesy of the Hoover Archives.

Rural women not only appeared in novel combinations in political posters, they were also represented in the larger-than-life format previously reserved only for workers and Red Army heroes. The magnification device had been used during

the Civil War but had receded from visual propaganda in the 1920s. Its reintroduction in the early 1930s served to accentuate the importance, once again, of superhuman Bolshevik heroes whose deeds made them Gullivers among the Lilliputians. In the system of signification of political posters, perspectival distortions served to identify heroic figures. Thus the *kolkhoznitsa* was now sometimes represented as a giant figure, towering over enemies and the landscape around her.

The 1930 poster "Peasant Woman, Join the Collective Farm!" ("Krest'ianka, idi v kolkhoz!") (printing of 20,000) depicts a larger-than-life young peasant woman resisting the tugs of a priest, drunkard, and kulak who seek to arrest her progress along the path leading to the *kolkhoz*.<sup>17</sup> Her severe and determined expression and her forceful gesture toward the collective farm make it clear that she is a person to be reckoned with. The formidable peasant woman heroically resisting "class enemies" in the countryside became a stock figure in visual propaganda of the early 1930s.<sup>18</sup>

Whereas earlier images of peasant women had often emphasized maturity and fecundity—broad hips and large bosoms—the new image stressed a far slimmer and more youthful body, with understated breasts. The *kolkhoznitsa* was depicted as a young and vigorous worker but seldom a mother.<sup>19</sup> Heroic status and youthfulness became inextricably intertwined.

A poster issued in 1930 drawn by the distinguished Soviet artist Aleksandr Deineka, "Collective Farmer—Be an Athlete" ("Kolkhoznik bud' fizkul'turnikom") (printing of 20,000), exemplifies the new image of the female body (Figure 5). The poster features two young women and a man doing calisthenics. All are barefoot, wearing exercise clothes (short skirts or shorts); the women have short hair (in the style of women workers) and trim bodies. Two figures are in the background: a man driving a tractor and a man drying himself with a towel. The tractor driver serves as a reminder that exercise is connected to work, that it enhances labor power.

Collectivization posters of the early 1930s seldom accentuated the fecundity of peasant women (signified by large bosoms and corpulence) or portrayed female peasants with their children. Most of the imagery depicted peasant women engaged in agricultural labor. The attributes of youth, agility, and fitness were directly linked to the labor function. As one contemporary reviewer remarked, political art should create the "image of joyful, lively, intense, creative labor." The image of the peasant woman focused attention on production, not reproduction.<sup>20</sup>

The traditional class marker for peasant women, the sickle, disappears in the

<sup>17</sup> RU/SU 1431.

<sup>18</sup> For another example of this type of poster, see "V nashem kolkhoze net mesta popam i kulakam," 1930 or 1931, RU/SU 1756.

<sup>19</sup> There were a few notable exceptions. The poster by Pichurgin discussed above shows two young children playing in the lower left corner of the composition. Images of children generally appeared only in posters devoted to social themes, such as communal dining and nurseries.

<sup>20</sup> *Produksiia izobrazitel'nykh iskusstv*, nos. 6–7 (1932): 10. It should be noted that a major effort was mounted during the First Five-Year Plan to recruit women, especially urban women, into the work force. This was not much of an issue in the countryside, since most peasant women already participated extensively in the rural economy.



FIGURE 5: A poster by Aleksandr Deineka, "Kolkhoznik bud' fizkul'turnikom" (Collective Farmer—Be an Athlete) (1930), in the collection of the Tretyakov Gallery, Moscow.

early 1930s (the scythe disappears in images of male peasants as well). The tractor takes its place. In fact, the tractor becomes a key signifier for collective farms in visual propaganda and a symbol of progress more generally. Many political posters featured tractors, and men often sat in the driver's seat. But in collectivization posters, women also make an appearance as tractor drivers. Out of 106 political posters relating to agriculture between 1930 and 1934 that include images of women, 37 (35 percent) depict women behind the wheel of a tractor. An occasional poster in 1929 had incorporated images of female tractor drivers (see Figure 2), but the connection between women and tractors was heavily emphasized only from 1930 onward.

THE EMERGENCE OF A NEW ICONOGRAPHY can only be explained by a combination of circumstances; no single factor will suffice to account for such a shift in the basic pattern of visual representation. At the outset, it is worth noting that female poster artists achieve prominence for the first time in the early 1930s, many of them concentrating particularly on the theme of collectivization. As we have seen, some of the most memorable posters on this theme with large printings came from female artists such as Korableva.<sup>21</sup> The presence of female artists certainly

<sup>21</sup> Other important female poster artists who created key posters on the theme of collectivization were Nataliia Pinus and Mariia Voron, whose work is discussed below.



deserves attention, but it cannot account for the prevalence and consistency of the new imagery. Many collectivization posters were created by male artists, who far outnumbered the women in the profession.

The shift in iconography coincided with momentous developments in the lives of rural women. When the collectivization campaign gathered momentum in late 1929 and early 1930, women became centrally involved in the growing resistance movement in the countryside. Resistance to forced collectivization was widespread in these months and indeed, throughout the rest of 1930 and into 1931; sometimes, large groups of peasants mobilized in violent encounters with local authorities.<sup>22</sup> Rural women displayed particularly vigorous opposition to the collective farms, and they often stood in the forefront of these rebellions.<sup>23</sup> As one contemporary observed, "A significant proportion of the mass of peasant women turned against collectivization."<sup>24</sup>

Official commentators ascribed the hostile attitudes of rural women to their susceptibility to rumors and agitation by kulaks.<sup>25</sup> In fact, women had many grievances against the new collective farm system. One major issue centered on the socialization (communal ownership) of livestock, an aspect of the peasant household economy and culture traditionally under female supervision. During the early months of forced collectivization in 1929 and early 1930, local authorities confiscated peasants' farm animals, particularly the precious cow—the *byrenushka* of Russian folklore—which provided milk for the children and often functioned as a ritual totem of the peasant household.<sup>26</sup>

Collectivization coincided with a vigorous campaign against institutionalized religion, and the establishment of the new farms often coincided with the closing of churches and suppression of religious activity in the countryside. The attack on churches and the clergy infuriated many peasants and galvanized women into mass resistance.<sup>27</sup> Women also feared rumored changes. Word had it that "collectivization would bring with it the socialization of children, the export of women's hair, communal wife-sharing, and the notorious common blanket under which all collective farmers, both male and female, would sleep." Not all these

<sup>22</sup> On this subject, see Robert Conquest, *The Harvest of Sorrow: Soviet Collectivization and the Terror-Famine* (New York, 1986), chap. 7.

<sup>23</sup> Conquest, *Harvest of Sorrow*, 152, 154–55, 157, 166; Merle Fainsod, *Smolensk under Soviet Rule* (New York, 1953), 253; Moshe Lewin, *The Making of the Soviet System: Essays in the Social History of Interwar Russia* (New York, 1985), 179; Lynne Viola, *The Best Sons of the Fatherland: Workers in the Vanguard of Soviet Collectivization* (New York, 1987), 105; Lynne Viola, "Bab'i bunty," *Russian Review*, 45 (1986): 23–42; Robert William Davies, *The Socialist Offensive: The Collectivization of Agriculture, 1929–1930* (London, 1980), 136–37; A. A. Novosel'skii, ed., *Materialy po istorii SSSR: Dokumenty po istorii Sovetskogo obshchestva* (Moscow, 1955), 327–67.

<sup>24</sup> U. Ulasevich, ed., *Zhenshchina v kolkhoze* (Moscow, 1930), 6.

<sup>25</sup> According to a contemporary author writing in 1930: "It must be said that the kulaks, by means of their agents—well-to-do and middle peasants—knew how to win over a significant proportion of the masses of women, pull them into the anti-kolkhoz movement, direct their dissatisfaction with the hard conditions of life against the new form of economic organization"; Ulasevich, *Zhenshchina v kolkhoze*, 7.

<sup>26</sup> Conquest, *Harvest of Sorrow*, 157; Lewin, *Making of the Soviet System*, 179. For a discussion of the significance of the cow in peasant religion, see B. A. Uspenskii, *Filologicheskie razyskaniia v oblasti slavianskikh drevnostei (Relikty iazychestva v vostochnoslavianskom kul'te Nikolaia Mirlikiskogo)* (Moscow, 1982), 118, 128.

<sup>27</sup> See Fainsod, *Smolensk*, 253–54, for an example of the involvement of local clergy in women's rebellions; Conquest, *Harvest of Sorrow*, 207; Viola, "Bab'i bunty," 29–30.

rumors seemed far-fetched to rural women, many of whom had observed a libertarian attitude toward sex on the part of some Communist Youth League activists and incidents of sexual impropriety by local party bosses.<sup>28</sup>

Faced with the destruction of their way of life, peasant women (and men) sought to explain their sudden and devastating misfortune with reference to two great calamities of the Russian popular consciousness, one historical and the other symbolic. According to the first, collectivization was a "second serfdom";<sup>29</sup> according to the other, it was the arrival of Antichrist and the beginning of the Apocalypse.<sup>30</sup> The collective farm, together with its tractors, became a symbol of Antichrist on earth. In late 1929, rumors began to circulate in many rural areas that "antichrist had arrived and that the world would soon come to an end."<sup>31</sup>

Female resistance to collectivization took active as well as passive forms. Not only did many women refuse to join the farms (even when, in some cases, their husbands did so), they also participated in riots in which they attacked and sometimes burned down the *kolkhoz* stables, barns, haystacks, and houses; they confiscated seeds, blocked and sometimes destroyed tractors, and attacked local officials. In other cases, women showed up at meetings on collectivization instead of the men, interrupted the proceedings and mounted a voluble protest.<sup>32</sup>

The authorities adopted a cautious and moderate response to peasant women who resisted collectivization, a circumstance that helps explain the predominantly female composition of the rebellions. In contrast to their male counterparts, the women who participated in violent collective actions were seldom accused of being kulak henchmen (*podkulachnitsy*), and relatively few were charged with counter-revolutionary crimes. Local authorities generally did not use force to contain women's protests and sometimes did not even report incidents to higher authorities. Even though women were arrested for their activities from time to time, they enjoyed an immunity from summary prosecution not extended to men.<sup>33</sup> Yet official indulgence was limited to acts of collective female resistance. Women enjoyed no special status when it came to the campaign to "liquidate the kulaks as a class." Entire families were expropriated and sent into exile, with no special dispensations for either women or children.<sup>34</sup>

<sup>28</sup> Viola, "Bab'i bunty," 31. Rural women were deeply hostile to the libertarian sexual attitudes and practices of Bolsheviks. On peasant women's attitudes toward sex, see Richard Stites, *The Women's Liberation Movement in Russia: Feminism, Nihilism, and Bolshevism, 1860-1930* (Princeton, N.J., 1978), 382.

<sup>29</sup> Conquest, *Harvest of Sorrow*, 152. Peasants referred to the VKP (All Union Communist Party) as the "second serfdom" (*vtoroe krepostnoe pravo*).

<sup>30</sup> Of course, the two were historically related. Peter the Great, who extended the system of serfdom, appeared to the popular imagination—particularly among Old Believers—precisely as Antichrist.

<sup>31</sup> Viola, *Best Sons of the Fatherland*, 105; Viola, "Bab'i bunty," 29-30.

<sup>32</sup> An example of this can be found in Maurice Hindus, *Red Bread: Collectivization in a Russian Village* (Bloomington, Ind., 1988), 45-47.

<sup>33</sup> As Conquest describes it: "For the 'women's rebellions,' according to one activist observer, came to follow definite tactics. First the women would lead the attack on the kolkhoz; 'if the Communists, Komsomols and members of the village Soviets and Committees of Unwealthy Peasants attacked them, the men rallied to the women's defence. This tactic aimed at avoiding intervention by armed forces, and it was successful.' In the Southern Ukraine, the Don and the Kuban, the collective farm structure had virtually collapsed by March 1930"; Conquest, *Harvest of Sorrow*, 158.

<sup>34</sup> Conquest, *Harvest of Sorrow*, chap. 6.

The official terminology for the peasant women's rebellions was *bab'i bunty*. The words themselves conveyed official assumptions about the actors and their actions and shed some light on government leniency toward the protesters. The term *baby* (singular *baba*) had, as noted earlier, strong pejorative connotations, especially for politically conscious women and men. *Bunt* referred to a particular type of mass action and carried the connotations of uncontrolled elemental rebellion or riot. The underlying implication was that ignorant, naïve, oppressed women were engaged in spontaneous and irrational protests.<sup>35</sup> The term implied an attitude that was both dismissive (the protests could not be taken seriously) and demeaning (the actors belonged to a category of the population so lowly that they were unworthy even of retribution).

Verbal and visual discourse in 1930 thus offers a study in contrasts. The pejorative characterization of women implicit in the term *bab'i bunty* differed sharply from the image of peasant women conveyed by political art. In fact, the new image of the female peasant represented the antithesis (and very deliberately so) of the previous imagery, which had carried associations with the much-maligned *baba*. This circumstance accentuates the complexity of the new imagery and invites a consideration of the conditions that account for the ubiquitous presence of the *kolkhoznitsa* in visual propaganda just when peasant women were presenting the authorities with formidable and unrelenting opposition to collectivization in the countryside.

The new image of the female peasant was polyvalent. It conveyed several meanings simultaneously and must be comprehended as a complex symbol. Viewed against the background of women's resistance to the *kolkhoz*, the new iconography of peasant women functioned in much the same way as Stalin's pronouncements did. Moshe Lewin has given this characterization of the role of Stalin's verbal discourse: "Stalin's method consisted of presenting his plans and wishes as accomplished fact, so as to encourage the Party organizations and the other sectors of the administration to come into line with the 'actual situation' as it allegedly existed 'everywhere else.'"<sup>36</sup> In a similar fashion, the smiling woman tractor driver appeared in posters not as an accomplished fact but as an indication of what should be, as an incentive to make it happen. The poster constituted a kind of incantation designed to conjure up the new woman who would perform certain roles in a specific spirit and manner.

Representations of the countryside as populated exclusively by vigorous young *kolkhoznitsy* and *kolkhozniki* may also have facilitated the imposition of agrarian policies in the early 1930s. Political art projected a rural world in which the *krest'ianka-baba*, together with traditional peasant customs and attitudes, no longer had any place. An old Russian proverb, originating in the countryside, was known to all: "A chicken is not a bird; a *baba* is not a human being."<sup>37</sup> Visual propaganda reinforced the old proverb by creating an image of the present-future in which

<sup>35</sup> Viola, "Bab'i bunty," 23.

<sup>36</sup> Moshe Lewin, *Russian Peasants and Soviet Power: A Study of Collectivization* (Evanston, Ill., 1968), 457.

<sup>37</sup> Another version goes like this: "A crab is not a fish—a woman is not a person." Susan Bridger, *Women in the Soviet Countryside: Women's Roles in Rural Development in the Soviet Union* (Cambridge, 1987), 6.

the *krest'ianka-baba* had been replaced by the youthful and enthusiastic *kolkhoznitsa* building socialism. The new world of the village depicted by Stalinist posters effaced virtually all aspects of the traditional peasant woman, her culture, her way of life. Within the context of a society undergoing forced collectivization, visual propaganda helped justify and make more palatable policies designed to reconstitute the countryside by brutal means.

HOW DID CONTEMPORARIES "READ" POLITICAL POSTERS associated with the collectivization campaign? Officials concerned with visual propaganda, as well as poster artists themselves, placed great importance on this question. In the 1930s, those engaged in poster production focused their attention on the effectiveness of propaganda and its capacity to convey the desired message. Although there had been some research conducted in the 1920s on viewer responses to art of various kinds, the 1930s marked the beginning of what might be called "scientific propaganda," an attempt to gauge viewer responses to posters and systematically evaluate audience reception.

The Central Committee resolution of March 11, 1931, addressed the issue of reception directly and called for specific measures to improve information concerning viewer responses.<sup>38</sup> These measures included the establishment of a journal devoted to poster reviews, creation of poster review committees among workers and peasants, and the founding of a new organization of poster artists.<sup>39</sup> All these proposals were rapidly implemented in the spring of 1931.<sup>40</sup>

Contemporaries concerned with the production of posters proceeded from the assumption that worker and peasant audiences required different styles of political art to accommodate the different ways of viewing visual propaganda in the cities and countryside. One artist argued in March 1931, "The city poster must cry out, be eye-catching, because here people only catch a glimpse of it, see it in passing. The peasant, on the contrary, loves to stop in front of a poster and examine it in all its particulars." A leading official in poster production observed that "the eye of the peasant finds it easiest to comprehend the *lubok* and gets lost in the details of the usual 'city' poster."<sup>41</sup> The application of traditional folk styles to Soviet posters was a controversial issue in the 1930s, when artists and officials were eager to create an entirely new "proletarian" style of political art. Despite the controversy, however, the *lubok* format, with such characteristic conventions as

<sup>38</sup> On this resolution, see above, page 58.

<sup>39</sup> *Brigada khudozhnikov*, nos. 2-3 (1931): 2-3, Articles 6, 7, 8 of the resolution.

<sup>40</sup> The first meeting of the review board under Izogiz took place on April 5, 1931, with representatives from five major Moscow factories. The meeting was devoted mainly to a discussion of 22 posters Izogiz intended to publish. Most were subjected to severe criticism and half were rejected outright. *Brigada khudozhnikov*, nos. 2-3 (1931): 3. The Union of Russian Revolutionary Poster Artists (ORRP) was established soon after the Central Committee Resolution, under the leadership of Dmitrii Moor. See Tsentral'nyi Gosudarstvennyi Arkhiv Literatury i Iskusstva (TsGALI), f. 1988, op. 1, ed. khr. 33 for a draft of the union's charter. The new journal, *Produksiia izobrazitel'nykh iskusstv* (later *Produksiia izo-iskusstv* and *Plakat i khudozhestvennaia reproduksiia*) was published by the Kritiko-bibliograficheskogo Instituta.

<sup>41</sup> *Brigada khudozhnikov*, nos. 2-3 (1931): 3, 4. The *lubok* was a folk-art print, often satirical, popular among the common people, both rural and urban, before the revolution.

contrasting panels showing “then and now” and “we and they,” was commonly utilized in the 1930s.

It might at first be assumed that posters on agricultural themes and especially those promoting collectivization were aimed at a rural audience. Certainly, some of these posters made their way to the countryside, where they hung in collective farm meeting rooms, reading rooms, and the headquarters for local party and government organizations. But, in the early 1930s, when chaos and disruption prevailed in most rural areas, it is far from clear how extensively visual propaganda penetrated the countryside or was even intended to penetrate the countryside.<sup>42</sup>

Judging by the content of these posters, a rural audience hardly seems appropriate. What did peasants in 1930 or 1931 see in a poster featuring tractors driven by *kolkhoznitsy* when the number of tractors in these years was still small? As of December 1930, 88.5 percent of the collective farms still had no tractors of their own; Machine Tractor Stations served only 13.6 percent of all collective farms.<sup>43</sup> Women drivers were an even greater oddity. They comprised only 6 percent of the tractor drivers in the country as a whole in 1932.<sup>44</sup> What kind of an impression did the image of a trim young peasant woman make on peasants, especially in 1932–1933, when millions were emaciated and dying in the famine? These and many other incongruities between the pictures and experience indicate that perhaps a different audience was intended.

Although further evidence is needed, I would argue that many of the posters relating to collectivization, especially between 1930 and 1932, were aimed at an urban and working-class audience to a far greater extent than at the rural population. The point of this visual propaganda was to generate support outside rural areas for state policies that were being imposed violently on the countryside. Images of the smiling, slender *kolkhoznitsa* that must have appeared grotesque in a rural context—especially in the midst of devastating famine—were perhaps more plausible in an urban setting, where the image of the peasant woman now shared common features with the image of the woman worker.

In fact, visual representations generated by Soviet artists during the collectivization campaign were primarily projected through an urban lens. The specific images discussed above, accentuating as they did certain body features and expressions, conveyed urban values and assumptions (slimness versus robustness, for instance) far more than rural ones. Even the emphasis on tractors played more effectively to an urban than a rural audience. Whereas workers easily comprehended the value of mechanization, “the reaction of peasants to the tractor was extremely varied.”<sup>45</sup> Many peasants were deeply hostile to tractors and mistrustful about their function. The contemporary press reported incidents in which

<sup>42</sup> According to Lewin, “The extreme efficacy of this [anti-collectivization] propaganda was due, not to the strength of the kulaks, but to the weakness of official propaganda, and above all to the distrust which the peasants felt, and were so frequently justified in feeling, for the government during the collectivization campaign and more especially during the autumn and winter of 1929–30”; *Russian Peasants and Soviet Power*, 487.

<sup>43</sup> Conquest, *Harvest of Sorrow*, 180.

<sup>44</sup> E. K. Kravchenko, *Krest'ianka pri sovetskoi vlasti* (Moscow, 1932), 46.

<sup>45</sup> Davies, *Socialist Offensive*, 384.



peasants destroyed tractors; others denounced them as "the work of Antichrist" and a return to serfdom.<sup>46</sup>

The inappropriateness of some posters for a rural audience was underscored in reviews of posters by students, officials, artists, and even workers and peasants. These reviewers often drew attention to the lack of authenticity in the representation of rural scenes. They complained that some Soviet artists had little knowledge of the countryside and created images that contained gross inaccuracies relating to machinery, terrain, people, clothing, labor activities, and animals. One poster showed tractors sowing in green fields; another had peasants working in winter without suitable winter clothes.<sup>47</sup> Reviewers concluded that peasants laughed at such posters and would not take them seriously. But these inaccuracies presented fewer problems if the audience was urban and not rural.

Some posters featuring young women were deemed positively dangerous, even counterrevolutionary. One reviewer of Korableva's "Come, Comrade, Join Us in the Collective Farm" (Figure 3)—possibly the most emblematic of the collectivization posters—harshly criticized what he saw as an implicit sexual invitation in the poster. In a vituperative review, he argued that the poster was having a "counterrevolutionary and harmful effect in the contemporary countryside because the second half of the text is written on the woman and, undoubtedly, will be used by kulak-agitators in the broadest possible way."<sup>48</sup> His point was that the phrase "Join us in the collective farm" happened to be printed across her midsection—by implication, a sexual invitation. The suggestion that *kolkhoznitsy* proffered sexual invitations had grave significance in the contemporary context, amplifying fears of a linkage between communalization of peasant property and the peasant woman's body.

The application of traditional styles and color symbolism, as well as other conventions drawn from religious and folk art, helped to make the unfamiliar familiar, to enhance the credibility of images for both urban and rural viewers. Soviet artists endeavored, by means of these devices, to present a novel image—for example, young peasant women and tractors—in a framework that was known to the viewers. Several techniques were used to enhance the familiarity and acceptability of such seemingly anomalous images as women tractor drivers.

One such device was color symbolism.<sup>49</sup> Known to most people from religious icons and more than a decade of Bolshevik propaganda, color symbolism was a basic part of learning to "read" posters. The 1930 poster "Peasant Women! Let Us Increase the Harvest! Let Us Unite Peasant Households in Collective Farms"

<sup>46</sup> For a discussion of peasant reactions to tractors, as reported in the contemporary press, see E. H. Carr and R. W. Davies, *Foundations of a Planned Economy, 1926–1929* (Harmondsworth, 1974), 225–27; Davies, *Socialist Offensive*, 384–85.

<sup>47</sup> *Za proletarskoe iskusstvo*, nos. 3–4 (March–April 1931): 8; *Produktsiia izo-iskusstv*, no. 2 (1932): 2. A meeting of collective and state farm workers to review posters under the auspices of the Bibliographic Institute criticized the depiction of a tractor because it consisted of parts of different tractor systems; *Produktsiia izo-iskusstv*, nos. 3–4 (1932): 20.

<sup>48</sup> *Brigada khudozhnikov*, nos. 2–3 (1931): 4.

<sup>49</sup> Contemporary poster artists, such as the masterful Dmitrii Moor, devoted a good deal of attention to color symbolism. In articles and speeches, he insisted on its importance in the semantic system of visual representation. See his article, "Oformleniui plakata nado uchi't'sia," in *Brigada khudozhnikov*, no. 4 (1931): 9, 29.

("Krest'ianki! Povysim urozhai! Ob"edinim krest'ianskie dvory v kollektivny") illustrates how this traditional artistic device was applied to new subject matter.<sup>50</sup> The smiling young *kolkhoznitsa* and her tractor are entirely red. A red person on a red tractor was scarcely a realistic rendering of the rural scene. But viewers knew how to interpret the color red and to appreciate its positive connotations, since red was a privileged color in both religious and Bolshevik art. It conferred sacred status on a person or object. Behind the smiling tractor driver in the poster are seven scenes showing the condition of women before collectivization and afterward. A before-and-after format was characteristic of the folk art style of the *lubok*, which was also familiar to most viewers.

Photomontage was another technique used to enhance the authenticity of images. Beginning in 1931 and 1932, Soviet artists increasingly used photomontage in posters relating to agricultural themes. Photographs of peasant women began to supplement paintings and illustrations, lending a documentary quality to some posters and providing material evidence, as it were, to support the illusions. This technique is illustrated by a poster in 1932 by Nataliia Pinus, "Collective Farm Women, Be in the First Ranks of the Fighters" ("Kolkhoznitsy, bud'te v pervykh riadakh boitsov") (printing of 30,000).<sup>51</sup> Here, a photograph of a confident and smiling *kolkhoznitsa* is combined with a painting of a collective farm silo and other buildings, producing a blend of reality and fantasy.

THE FIRST FIVE-YEAR PLAN lasted four years and three months. When the plan ended on December 31, 1932, the country was in the midst of a massive famine that claimed millions of lives in agricultural regions of the country. Against this background, the regime celebrated the success of collectivization.<sup>52</sup> By 1933, over four-fifths of all the cultivated land in the Soviet Union had been collectivized and so had nearly two-thirds of the peasant households.<sup>53</sup> The Seventeenth Party Congress, held in February 1934 and proclaimed the "Congress of Victors," celebrated the victory of socialism.<sup>54</sup>

The year 1934 has been described as the beginning of the "great retreat" from social, economic, and cultural policies introduced during the "socialist offensive" of 1929–1933. Nicholas Timasheff coined the phrase "great retreat" to describe the restoration of some ideas and practices predating the First Five-Year Plan or even the 1917 revolution.<sup>55</sup> The result was an amalgam of the old and the new,

<sup>50</sup> RU/SU 1684. There are many other examples of posters in which figures or objects are entirely red. See note 18 and "Krest'ianka, kollektivizirui derevniu," 1930, showing a row of red tractors and red women driving them (printing of 40,000); KP p4.xxvi.7/1k krest'ianka.

<sup>51</sup> KP p4.xxvi.2/1r. The entire text reads: "Collective farm women, be in the front ranks of the fighters for the second five-year plan, for the construction of a classless society."

<sup>52</sup> In January 1933, Stalin announced that the "'economic foundation of a socialist society' had now been built and asserted that 'we have established the principle of socialism in all spheres of the economy by expelling the capitalist elements from it.'" Robert C. Tucker, *Stalin in Power: The Revolution from Above, 1928–1941* (New York, 1990), 213.

<sup>53</sup> Alec Nove, *An Economic History of the U.S.S.R.* (1969; Middlesex, Eng., 1982), 174.

<sup>54</sup> Nove, *Economic History of the U.S.S.R.*, 248.

<sup>55</sup> Nicholas S. Timasheff, *The Great Retreat: The Growth and Decline of Communism in Russia* (New York, 1946). Timasheff argues that major policy shifts took place that restored prerevolutionary ideas and practices relating to nationalism, hierarchy, and social and cultural life.

within the overall framework of the Stalinist party-state. In agriculture, the "great retreat" led to promulgation of the Model Charter for Collective Farms in 1935, supplanting the far more draconian version of 1930.<sup>56</sup> The 1935 charter eased various regulations and restored small personal farming on plots allotted to *kolkhozniki*, while retaining the basic organization of collectivized agriculture imposed in the early 1930s.

Political art witnessed its own version of a "great retreat" beginning in 1934. Toward the end of 1933, artists and critics expressed dissatisfaction with the prevailing canons for visual representation of the countryside. A review of a 1933 poster—Nataliia Pinus's "Collective Farm Woman, Be a Shock Worker of the Harvest" ("Kolkhoznitsa, bud' udarnitse uborki") (printing of 30,000)<sup>57</sup>—focused attention on the inadequacies of the "typicalization," or *tipazh*. The term referred not to typicality but to the correct rendering of a social category as it would appear in the unspecified future.<sup>58</sup> The reviewer disapproved of the artist's picture of two young, trim, and energetic *kolkhoznitsy* (one smiling, one serious) on their way to the field with rakes on their shoulders. He observed that "in the choice of *tipazh*, the artist Pinus wanted to present healthy, jolly, pretty, and intellectual faces, to show the new person who combines in herself physical strength and energy with a high level of culture. But it must be recognized that the artist did not succeed. The *kolkhoznitsy* are not typical. In the poster, we have instead some kind of 'Mashen'ka and Dasha,' pretty and rosy but completely uncharacteristic of the *kolkhoz* masses."<sup>59</sup> In essence, the reviewer criticized the artist for creating an image designed for urban viewers. In the aftermath of the First Five-Year Plan, the aim of official propaganda was shifting, and posters on rural themes were increasingly measured by their effectiveness in reaching and persuading a rural audience, now that the great majority of peasants had been collectivized.<sup>60</sup>

If we compare political posters produced in 1934 with those of 1930 or 1931, we can see some important modifications in the representation of the peasant woman. The *kolkhoznitsa* had begun to fill out and acquire a fuller, more rounded look. The large bosoms and corpulence of the 1920s did not return, but the trim and athletic look of the early 1930s also faded from the scene.<sup>61</sup> Collective farm women were still generally depicted as youthful, but now older, more mature

<sup>56</sup> For details on these changes, see Lazar Volin, *A Century of Russian Agriculture: From Alexander II to Khrushchev* (Cambridge, Mass., 1970), 244 and following.

<sup>57</sup> KP p4.xxvi.7/1k.

<sup>58</sup> As A. Lunacharskii remarked in an article in 1931: "artists should not only describe what is, but should go farther, to show those forces which are not yet developed, in other words, from the interpretation of reality it is necessary to proceed to the disclosure of the inner essence of life, which comes out of proletarian goals and principles"; *Brigada khudozhnikov*, nos. 5–6 (1931): 13.

<sup>59</sup> G. Martynov, "Plakaty k uborochnoi kampanii," *Produksiia izo-iskusstv*, no. 9 (September 1933): 7. Mashen'ka and Dasha are diminutive forms of common female Russian names.

<sup>60</sup> At the end of 1934, the journal that reviewed political posters reported on a gathering of collective farmers at the Kalinin Collective Farmers' Club to view and criticize posters on the theme of livestock management. The farmers included young and old, activists and leaders as well as rank-and-file farmers, men and women. This event and the report on it provide evidence for the growing concern with the *kolkhoz* audience. See A. Unkovskii, "Kolkhozni smotr sel'skokhoziaistvennykh plakatov," *Plakat i khudozhestvennaia reproduksiia*, no. 12 (December 1934): 16–17.

<sup>61</sup> See below, page 79, for a discussion of Vera Mukhina's famous statue of 1937, "Worker [Male] and Collective Farm Woman" ("Rabochii i kolkhoznitsa"), which partially restored the image of an athletic peasant woman.

women occasionally made an appearance.<sup>62</sup> Smiles and quiet satisfaction became far more prevalent than before, and few posters conveyed the grimness and determination characteristic of *kolkhoznitsy* in the early 1930s. The new demeanor of collective farm women disconcerted some reviewers, who criticized posters for failing to show the *kolkhoznitsa*'s "intensive struggle for socialist reconstruction of the countryside, resistance to the class enemy, resolute surmounting of obstacles, enthusiasm for collective labor and its pathos."<sup>63</sup> But in 1934, especially after the Congress of Victors, rural propaganda shifted its emphasis from struggle and confrontation to serenity and joyfulness amid abundance. Earlier posters characteristically showed peasant women engaged actively in work. Now they were often depicted in a contemplative or celebratory mood as they surveyed the results of collective farm labor. Symbols of prosperity abound: sheaves of wheat, plump and well-groomed farm animals—especially the venerable cow—and fields redolent with crops.<sup>64</sup>

Color symbolism also changed. As the 1930s progressed, pastel colors, especially blues and greens, as well as white and yellow, were substituted for the harsher tones of red and black that dominated earlier collectivization posters. A fascinating report of collective farmers' reactions to posters in late 1934 noted that the viewers displayed a strong preference for soft muted colors and were especially partial to one poster with a "delicate blue background." They reacted negatively to bright garish colors. According to this report, the collective farmers paid attention to color and imagery and generally ignored the text.<sup>65</sup>

Although most *kolkhoznitsy* in posters from 1934 still wore a kerchief tied at the back of the neck in the fashion of urban women, their clothing was sometimes more decorative than previously and included traditional touches, like an apron, which had been part of the standard image in the 1920s but disappeared in the early 1930s. Mariia Voron's striking poster, "Shock Work at Harvest Befits a Bolshevik Harvest" ("Udarnuiu uborku—bol'shevistskomu urozhaiu") (printing of 60,000), shows the complex combination of details that characterized rural imagery during the Second Five-Year Plan (Figure 6).<sup>66</sup> The *kolkhoznitsa*'s appearance (especially her stoutness, the blue color of the dress, and her apron) as well as her almost classically serene and motionless pose signify departures from visual conventions of the early 1930s. Except for the text, the only red in the picture is her red kerchief, tied behind her head. A contemporary reviewer of the poster

<sup>62</sup> See, for example, the poster by Iurii Alferov and M. Sokolov, "Uspekhi kollektivizatsii—torzhestvo ucheniia Lenina i Stalina" (1934), KP p4.ix.3.g. This poster was widely reproduced and appeared in ten different national languages.

<sup>63</sup> These comments were made on the poster by Alferov and Sokolov, "Uspekhi kollektivizatsii." It features two *kolkhoznitsy*, one younger, one older. The younger one holds a book by Stalin. *Plakat i khudozhestvennaia reproduksiia*, no. 7 (July 1934): 4.

<sup>64</sup> See, for example, "Udarnuiu uborku—bol'shevistskomu urozhaiu" (1934), RU/SU 1843, showing a sturdy peasant woman holding sheaves of wheat; in the background, the abundant harvest is being gathered. Viktor Ivanov, "Kolkhozniki, organizuite molochnye-tovarnye fermy" (1935), KP pk5.ix.9/1.k; "Vpered k dalneishemu razvitiu zhivotnovodstva!" (1936), KP p5.ix.12/1.v. Cows depicted in these posters belonged to the farm, although very limited private ownership of livestock was permitted according to the terms of the Model Charter of 1935. See also "Udarnuiu uborku," RU/SU 1843; "Po-Bil'shovits'komu borotit' za visokii urozhai!" (1937) (printing of 20,000), KP p5.ix.7/1p.

<sup>65</sup> See above, note 60.

<sup>66</sup> RU/SU 1843.



FIGURE 6: A poster by Mariia Voron, "Udarnuiu uborku-bol'shevistskomu urozhaiu" (Shock Work at Harvest Befits a Bolshevik Harvest) (1934), courtesy of the Hoover Archives.

gave the artist high marks for *tipazh* and praised the "romantic solemnity" of the image of the *kolkhoznitsa*.<sup>67</sup>

<sup>67</sup> *Plakat i khudozhestvennaia reproduksiia*, no. 9 (September 1934): 16.



An official campaign mounted in 1934 promoted personal cleanliness and attractive clothing.<sup>68</sup> When a new glossy magazine—called “At the Construction Site of the Machine Tractor Station and State Farm” (*Na stroike MTS i sovkhovozov*)—appeared in June 1934, it heralded with pictures and accompanying text “the new life” in the countryside.<sup>69</sup> This slick propaganda organ, which attracted the services of Maxim Gorky and El Lissitzky, asserted in a later issue that “our rural youth are not dressed any worse than city youth. Many of our girls have begun to wear silk dresses, velveteen and fine wool coats with fur collars, berets. The men wear good suits, shoes, and always a necktie.”<sup>70</sup> Political artists became more attentive to clothing and depicted peasant women wearing pretty blouses or dresses, sometimes accentuated by decorative articles such as embroidered scarves, even while at work.<sup>71</sup> The inclusion of traditional Russian folk-style embroidery—inconceivable during the early 1930s—reflected the nationalist and folk revival then under way.

A poster by Konstantin Zotov in 1934 exemplifies some of the changes taking place in the semantic system of visual propaganda. Zotov’s poster (printing of 60,000) depicted a peasant family—a mother, father, and toddler—joyfully gathered around a gramophone. The only text is a quotation from Stalin: “Any peasant-collective farmer or individual farmer now has the possibility to live in a humane manner, if he only wants to work honestly and doesn’t loaf, is not a vagrant, and doesn’t plunder *kolkhoz* property.”<sup>72</sup> What is unusual about the poster is that it emphasizes, at least implicitly, the *kolkhoznitsa*’s achievements in both production (she is presumably a shock worker) and reproduction (she is a mother). Moreover, the poster presents collective farm workers at leisure rather than at work.<sup>73</sup> The labor process has been replaced by the fruits of labor, in the form of a gramophone, other household belongings, and personal attire.<sup>74</sup>

<sup>68</sup> As *Pravda* and *Komsomol'skaia Pravda* stated: “We endorse beauty, smart clothes, chic coiffures, manicures . . . Girls should be attractive. Perfume and make-up belong to the ‘must’ for a good Comsomol girl . . . Clean shaving is mandatory for a Comsomol boy.” Cited in Timasheff, *Great Retreat*, 317. Various posters were devoted to the theme of cleanliness, such as the one by Lodygin, “Otkrytoe pis'mo ko vsei kolkhoznnoi obshchestvennosti,” 1934, which appeared in a printing of 100,000. The large size of the press run indicates the importance attributed to this issue.

<sup>69</sup> “Muzhik i kolkhoz,” *Na stroike MTS i sovkhovozov*, no. 1 (1934).

<sup>70</sup> *Na stroike MTS i sovkhovozov*, no. 1 (1935): 7.

<sup>71</sup> Two posters of the *kolkhoznitsa* and a cow, issued in 1935 and 1936 respectively, exemplify this development. Each poster shows a peasant woman wearing a colorful outfit complemented by a white embroidered scarf tied around her neck and stylishly draped over her chest. See Viktor Ivanov, “Kolkhozniki, organizuite molochno-tovarnye fermy!” which appeared in the fall of 1935 (printing of 75,000), KP pk5.ix.9/1.k. A similar poster by Petr Karachentsov appeared in 1936, “Vpered, k dal'neishemu razvitiu zhivotnovodstva!” (printing of 50,000). A quote from Stalin appears at the top of the poster: “The combination of the personal interests of the collective farmers and the general interests of the collective farm—that is the key to the strengthening of the collective farms.” KP p5.ix.12/1.v.

<sup>72</sup> KP p4.ix.1.b. The poster and a review of it appear in *Plakat i khudozhestvennaia reproduksiia*, no. 7 (July 1934): 10–11.

<sup>73</sup> Contemporary newspaper and journal articles devoted to the lives of outstanding individual farmers and workers (*udarniki* and later Stakhanovites) presented similar scenes of private life. See, for example, *Na stroike MTS i sovkhovozov*, no. 3 (1934): 3, for accounts of women production heroes. One is shown reading a newspaper. For a discussion of the presentation of Stakhanovite workers, see Lewis H. Siegelbaum, *Stakhanovism and the Politics of Productivity, 1935–1941* (Cambridge, 1988), chap. 6.

<sup>74</sup> This was not the only poster produced in 1934 focusing on the relationship between labor and

Both husband and wife in the poster are well dressed: she in a pink blouse and he in a black jacket with a peasant-style shirt, embroidered at the collar. He is strikingly handsome; she is lovely to look at and, with her gay smile, might just as well have appeared in a commercial advertisement for gramophones! A contemporary critic, reviewing the poster, considered the *tipazh* of the *kolkhoznitsa* the most successful of the three figures.<sup>75</sup> The child is plump, cute, and claps his hands in delight at the music (the hands also suggest a prayerful position). Mothers and children can seldom be found in collectivization posters of the early 1930s and then only in those explicitly devoted to the theme of social services. Even during the Second Five-Year Plan, very few posters on general themes show the peasant woman with her child, although such images did occasionally appear in contemporary magazines.<sup>76</sup>

The family is shown listening to the gramophone. Behind them are arrayed two items that signified a cultured and comfortable life in the countryside: an electric light and a shelf of books.<sup>77</sup> The reviewer criticized Zotov's representation of the peasant home as failing to contrast adequately the "bright, clean, cozy, and spacious home" of the contemporary peasant with the hut of the pre-collectivization era.<sup>78</sup> The figures, he thought, were too close together and gave the impression of lack of space in the dwelling. Visual propagandists devoted a good deal of attention to the issue of rural prosperity in 1934 and 1935. As presented in posters and magazine articles, accoutrements of the good life included a samovar, sewing machine, camera, bicycle, watch, musical instruments, and such home furnishings as "rugs, soft furniture, a dresser with a mirror, a radio, flowers, and lace curtains."<sup>79</sup>

A change also took place in the visual syntax used to depict the *kolkhoznitsa* during the Second Five-Year Plan. In contrast to the early 1930s, peasant women appeared more often in pairs or even in groups, without the presence of men. The 1934 poster by Aleksei Sittaro, "Toward a Prosperous Cultured Life" ("K zazhitochnoi kul'turnoi zhizni") (30,000),<sup>80</sup> shows five collective farm women (three agricultural workers, a mechanic, and a teacher) cheerfully striding forward carrying farm animals, wheat, books, and a wrench. They are robust—obviously well fed—and joyous.<sup>81</sup> The clustering of peasant women may have

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material acquisitions. See also the poster by Viktor Govorkov, "Skol'ko vesiat trudodni" (printing of 30,000). It showed what collective farmers could acquire with their labor, including livestock, clothing, and household furnishings. *Plakat i khudozhestvennaia reproduksiia*, no. 12 (December 1934): 14–15.

<sup>75</sup> *Plakat i khudozhestvennaia reproduksiia*, no. 7 (July 1934): 11.

<sup>76</sup> See, for example, *Na stroike MTS i sovkhov*, no. 1 (1935).

<sup>77</sup> The books on the shelf are by Maxim Gorky, Lenin, and Stalin, and there is a work on agricultural techniques.

<sup>78</sup> *Plakat i khudozhestvennaia reproduksiia*, no. 7 (July 1934): 11.

<sup>79</sup> In this connection, see Govorkov, "Skol'ko vesiat trudodni," cited in note 75, and the review of it in *Plakat i khudozhestvennaia reproduksiia*, no. 12 (December 1934): 15. The quotation is from *Na stroike MTS i sovkhov*, no. 1 (1935): 10.

<sup>80</sup> KP p4.xxvi.7/1.k.

<sup>81</sup> Other examples of posters with women only: N. Pinus, "Kolkhoznitsa, bud' udarnitse uborki urozhai" (printing of 30,000) (1933), KP p4.xxvi.7/1.k; Alferov and Sokolov, "Uspekhi kollektivizatsii" (1934), KP p4.ix.3.g; "Ide vesna, parue den', v kolgopsi nasha sila, tsvite kraina moloda, mogutnaia i shchasliiva" (printing of 20,000) (n.d. but probably mid-1930s), KP p4.ix.g.ide.

suggested to viewers that *kolkhoznitsy* had earned, perhaps for the first time, full confidence from the authorities, not just as individuals but as a social collectivity.

Beginning in 1933, some political posters emphasized the linkage between the *kolkhoznitsa* and Stalin.<sup>82</sup> Occasionally, his presence was signified by a quotation or a book.<sup>83</sup> Other times, he appeared in the background, for example, as a silhouette on a red flag.<sup>84</sup> In some posters, he is vividly featured alongside the peasant woman. One early example of the foregrounding of Stalin is a 1933 poster, "Women in the Collective Farms" ("Zhenshchiny v kolkhozakh"), produced in a press run of 40,000.<sup>85</sup> The poster is divided into two vertical segments: on the left, a photograph of Stalin addressing an audience of collective farm workers; on the right, a drawing of a *kolkhoznitsa* driving a tractor. She is in her mid-twenties, wearing a white kerchief tied behind her head and a cheerful cotton blouse with a red geometric pattern. Her expression might be described as quiet concentration. There is no hint of struggle, anger, or intensity.

Two years later, in 1935, Izogiz put out a poster by Iurii Tsishevskii, "Expand the Ranks of the Stakhanovites of Socialist Agriculture!" ("Shire riady stakhanovtsev sotsialisticheskikh polei!") (Figure 7).<sup>86</sup> It was published in a press run of 200,000—an extraordinary number for that year and an indication that the authorities considered it an important poster. The centerpiece of the poster is Mariia Demchenko, a Stakhanovite *kolkhoznitsa* who wrote to Stalin promising to achieve a record in the harvesting of beet root.<sup>87</sup> A woman in her early to mid-twenties with a kerchief tied behind her head, she stands in a field, holds a red banner, and smiles modestly while gesturing toward her accomplishments and toward Stalin. In the upper left-hand corner hovers a sketch of Stalin reading Demchenko's letter in *Pravda*; the text is reprinted below him.

In August 1939, a major agricultural exhibition opened in Moscow, providing the occasion for a series of posters on rural themes. P. Iastrzhembskii created a poster (50,000 were printed) commemorating the exhibition. It shows Stalin's image on a red flag above a statue of two collective farm workers (male and female);<sup>88</sup> the pedestal is covered with a stylized drawing of fruit; in the background is a photomontage of tractors against a light blue sky.<sup>89</sup> The composition introduces neo-classical overtones, but the message remains the same: the exemplary *kolkhoznitsa* and Stalin are inextricably intertwined. Similar

<sup>82</sup> On the development of the cult of Stalin in the 1930s, see the illuminating discussion in Tucker, *Stalin in Power*, esp. chaps. 7 and 20. For other examples of the visual connection between Stalin and exemplary collective farm workers, see *Na stroike MTS i sovkhovov*, no. 1 (1934); no. 2 (1934); no. 5 (1935); no. 1 (1936); no. 3 (1936).

<sup>83</sup> In Alferov and Sokolov, "Uspekhi kollektivizatsii—torzhestvo ucheniia Lenina i Stalina," the younger woman carries a red book prominently inscribed with author and title: I. Stalin, *Speech at the Congress of Collective Farm Shock Workers*. See above, note 62.

<sup>84</sup> See the frontispiece of *Na stroike MTS i sovkhovov*, no. 2 (1934), which shows a smiling woman tractor driver. A banner on the tractor reads "For a rich harvest," and there are silhouettes of Lenin and Stalin.

<sup>85</sup> KP p4.xxvii.b.1.zh.

<sup>86</sup> KP p5.ix.5/1/shire.

<sup>87</sup> Demchenko was widely celebrated. She provided the centerpiece for the first 1936 issue of *Na stroike MTS i sovkhovov*.

<sup>88</sup> This sculpture very much resembles the famous creation of Vera Mukhina discussed below, except that the male worker is replaced by a male peasant.

<sup>89</sup> RU/SU 1832.



FIGURE 8: Statue by Vera Mukhina, "Rabochii i kolkhoznitsa" (Worker [Male] and Collective Farm Woman) (1937), currently located at the Exhibition of Economic Achievements (VDNKh), Moscow.

(female) and by implication, between urban and rural spheres of Soviet society.<sup>94</sup>

In terms of both syntax and lexicon, Stalinist iconography expressed the domination of the cities over the countryside. Collectivization posters of the early 1930s were most likely directed at an urban audience and expressed quintessentially urban values concerning the body, the nature of labor, and the role of mechanization. Images of tractors, women, and hard work helped affirm the logic behind collectivization and provide a justification for the terror and famine inflicted on the rural population in the name of progress.

The focus of visual propaganda with agrarian themes shifted to the countryside after the First Five-Year Plan. The *kolkhoznitsa* was not featured to the same extent as earlier, although she still appeared in memorable posters produced in large numbers. The "great retreat" in political art meant a recasting of the image of the collective farm woman to incorporate elements of the semantic traditions of both the *krest'ianka* and the *kolkhoznitsa*. Nevertheless, an urban vision of rural life persisted. As the decade progressed, rural propaganda came to express more and more vividly the dream-like quality of the Stalinist utopia, with well-fed joyous peasants in fields of plenty.

During the Civil War and the 1920s, images of social groups functioned as abstractions. Everyone knew that not all workers were blacksmiths with a hammer and not all peasant women carried a sickle. These images were symbolic, intended to capture an element of what it meant to be a worker or female peasant. In the early 1930s, a new type of image appeared in visual propaganda, an image that served as a model, as an ideal type. This was the meaning of *tipazh*, the problem of typicalization that so concerned contemporary reviewers of political art.

The image of the *kolkhoznitsa* was not supposed to be realistic. Its purpose was to provide a visual script and an incantation, engendering a powerful illusion. To depict the rural woman was to invoke her. The image became a vehicle for anticipating and achieving the future. Stalinist propaganda created, in sum, a new political mythology. The picture, especially with the use of photomontage, acquired an unprecedented verisimilitude, not with the existing society but with the rural social world of the imagined future.

The visual language used to represent the *kolkhoznitsa* during the second half of the 1930s prefigured the pastoral romance of high Stalinism following World War II. Political posters issued between 1946 and 1953 often show plump, joyous women wearing embroidered blouses while standing at the wheel of a tractor or combine.<sup>95</sup> The sheaves of wheat have gotten even bigger, and symbols of

<sup>94</sup> Joan Scott argues that "gender codes" help establish and "naturalize" relations of domination. According to Scott, "middle-class reformers in [nineteenth-century] France . . . depicted workers in terms coded as feminine (subordinated, weak, sexually exploited like prostitutes) . . . [whereas] labor and socialist leaders replied by insisting on the masculine position of the working class (producers, strong, protectors of their women and children)." Joan Wallach Scott, *Gender and the Politics of History* (New York, 1988), 48. Eric Naiman argues that, during the 1920s, the New Economic Policy was sometimes discredited by critics applying a gender code that associated the NEP with feminine attributes. Eric Naiman, "Sexuality and Utopia: The Debate in the Soviet 1920s" (Ph.D. dissertation, University of California, Berkeley, 1991).

<sup>95</sup> See, for example, I. Krupskii and N. Rodin, "Bol'shevistskii urozhai soberem vo-vremia i bez poter'!" (1951) (printing of 60,000), produced in Kiev. KP p9.3.v.02.8/Bol'shevistskii. During World War II, women often replaced men in the *kolkhoz* and, for the first time, took over on a large scale men's jobs such as tractor driving.



prosperity are everywhere.<sup>96</sup> As before, the older *kolkhoznitsa* is accorded a place in some posters, alongside younger collective farm workers. The young ones, meanwhile, have grown more womanly, with full breasts and the healthy look of breeding cows. The color red retains its sacred aura, but now it is used more sparingly as a contrast to bright blue skies, golden wheat fields, and green pastures. Stalin looks on with avuncular approval as young pioneers and zealous collective farmers sing his praises. This is the bucolic bliss anticipated by 1930s iconography—a rural world populated by happy peasants who, in their spare time, perform folk dances in front of the Kremlin.<sup>97</sup>

<sup>96</sup> See, for example, B. Semenov, "Bor'ba za vysokii urozhai—Bor'ba za mir!" (1952). This poster was produced in Sverdlovsk; KP p9.3.v.02.b/bor'ba. V. Khachikian's poster, "Sobrat' urozhai v srok i bez poteri" (1953), appeared in various languages, including Armenian. KP p9.3.v.o2.b/sobrat'.

<sup>97</sup> See P. Golub' and A. Chernov, "Pogliadi: poet i pliashet/Vsia sovetskaia strana . . ." (1946) (printing of 200,000). KP p8.B1/pogliadi. This poster shows a woman in a Russian embroidered blouse and red dress, high-heeled white shoes and socks, dancing traditional Russian folk dances with two men, also in folk costumes. The crowd behind hold red flags, honoring May Day. In the background is the Kremlin Tower.

Maritime Asia, 1500–1800: The Interactive Emergence  
of European Domination

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JOHN E. WILLS, JR.

THE QUINCENTENNIAL FLOOD OF WRITING and arguing about Columbus and everything he can possibly be taken to stand for has involved surprisingly little attention to the “Indies” he hoped to find—the Spice Islands and, more broadly, the whole network of Asian maritime trade that linked the spice-producing regions with China, India, the Muslim heartlands, and indirectly with Europe. The opening of direct European voyages to Asia does not represent a world-historical transformation as fundamental as the establishment of the transatlantic routes but rather a reshaping of connections that had been developing for many centuries. Still, in the centuries that followed the arrival of Vasco da Gama in Calicut in 1498, the continued expansion and interweaving of maritime connections within and between the hemispheres was a basic factor in the creation of what Immanuel Wallerstein and others have called the modern world-system. The contributions of maritime Asia to this evolution are not nearly as thoroughly studied or as well integrated into theories like Wallerstein’s as those of the American connections. The remarkable surge of good writing on aspects of the events represented by the list of books under review published since 1987 makes this field accessible as never before, both to scholars who may wish to touch on it in teaching and summary writing and to those who are open to the challenges of the many opportunities for research and interpretation in an area of study that still is far from being fully developed.

The historiographical transformation of these studies in recent decades can be suggested by a change of name. Around 1960, when I began studying them, they were part of the “history of European expansion.” Today, the phrase “history of maritime Asia” is more acceptable and more indicative of the nature of thinking and writing in this specialty. Where previously even Asian nationalist scholars saw a rapid shift to European dominance of the seas in the “Vasco da Gama epoch,”<sup>1</sup> and saw the Asians largely as passive victims, today’s historians see that participants in the maritime history of every part of Asia included Asian navigators, merchants, pirates, investors, and merchant-princes, and that these Asian participants remained effective competitors of the Europeans far longer than earlier scholars had thought. Thus the long drift to European hegemony in Asian waters

<sup>1</sup> K. M. Panikkar, *Asia and Western Dominance: A Survey of the Vasco da Gama Epoch of Asian History, 1498–1945* (London, 1954).

James D. Tracy, ed., **The Rise of Merchant Empires: Long-Distance Trade in the Early Modern World** (Cambridge: Cambridge University Press, 1990), xviii + 442 pp.

James D. Tracy, ed., **The Political Economy of Merchant Empires: State Power and World Trade, 1350–1750** (Cambridge: Cambridge University Press, 1991), vii + 504 pp.

Jonathan I. Israel, **Dutch Primacy in World Trade, 1585–1740** (Oxford: Clarendon Press, 1989), xxi + 462 pp.

Geoffrey Parker, **The Military Revolution: Military Innovation and the Rise of the West, 1500–1800** (Cambridge: Cambridge University Press, 1988), xvii + 234 pp.

C. A. Bayly, **Imperial Meridian: The British Empire and the World, 1780–1830** (London: Longman, 1989), xv + 295 pp.

C. A. Bayly, **Indian Society and the Making of the British Empire** (Cambridge: Cambridge University Press, 1988), xi + 230 pp.

Roderick Ptak and Dietmar Rothermund, eds., **Emporia, Commodities and Entrepreneurs in Asian Maritime Trade, c. 1400–1750** (Stuttgart: Franz Steiner, 1991), xiii + 509 pp.

K. N. Chaudhuri, **Asia before Europe: Economy and Civilisation of the Indian Ocean from the Rise of Islam to 1750** (Cambridge: Cambridge University Press, 1990), xviii + 477 pp.

Denys Lombard and Jean Aubin, eds., **Marchands et hommes d'affaires asiatiques dans l'Océan Indien et la Mer de Chine 13<sup>e</sup>–20<sup>e</sup> siècles** (Paris: Editions de l'Ecole des Hautes Etudes en Sciences Sociales, 1988), 375 pp.

M. N. Pearson, **Before Colonialism: Theories on Asian-European Relations 1500–1750** (Delhi: Oxford University Press, 1988), 86 pp.

Ashin Das Gupta and M. N. Pearson, editors, **India and the Indian Ocean, 1500–1800** (Calcutta: Oxford University Press, 1987), xi + 363 pp.

M. N. Pearson, **The Portuguese in India** (Cambridge: Cambridge University Press, 1987), xx + 178 pp.

seems less overdetermined, less a foregone conclusion, much more multi-causal and contingent, and a much more challenging and rewarding area of study.

In the works here discussed is an even more interesting paradigm shift, in which Asian patterns of production, trade, and governance are seen to have fundamentally shaped the long process of emergence of the Asian maritime facets of the modern world-system. Many important aspects of European-Asian interaction in maritime Asia cannot be understood if we maintain an analytic separation of European intrusion and Asian response; they emerged in highly contingent and specific ways from the interactions, the congruences and mutual

Sanjay Subrahmanyam, **The Political Economy of Commerce: Southern India, 1500–1800** (Cambridge: Cambridge University Press, 1990), x + 401 pp.

Sanjay Subrahmanyam, ed., **Merchants, Markets, and the State in Early Modern India** (Delhi: Oxford University Press, 1990), viii + 276 pp.

Sanjay Subrahmanyam, **Improvising Empire: Portuguese Trade and Settlement in the Bay of Bengal 1500–1700** (Delhi: Oxford University Press, 1990), xix + 269 pp.

P. J. Marshall, **Bengal: The British Bridgehead: Eastern India 1740–1828** (Cambridge: Cambridge University Press, 1987), xv + 195 pp.

Hameeda Hossain, **The Company Weavers of Bengal: The East India Company and the Organization of Textile Production in Bengal, 1750–1813** (Delhi: Oxford University Press, 1988), xvii + 211 pp.

Susan Bayly, **Saints, Goddesses, and Kings: Muslims and Christians in South Indian Society, 1700–1900** (Cambridge: Cambridge University Press, 1989), xv + 504 pp.

Anthony Reid, **Southeast Asia in the Age of Commerce, 1450–1680, Volume 1: The Lands below the Winds** (New Haven, Conn.: Yale University Press, 1988), xvi + 275 pp.

Leonard Blussé, **Strange Company: Chinese Settlers, Mestizo Women, and the Dutch in VOC Batavia** (Leiden: KITLV, 1988), xiii + 302 pp.

Vicente L. Rafael, **Contracting Colonialism: Translation and Christian Conversion in Tagalog Society under Early Spanish Rule** (Ithaca, N.Y.: Cornell University Press, 1988), xiii + 230 pp.

N. Standaert, **Yang Tingyun, Confucian and Christian in Late Ming China: His Life and Thought** (Leiden: E. J. Brill, 1988), xi + 263 pp.

John Whitney Hall, ed., James L. McClain, assistant ed., **The Cambridge History of Japan, Volume 4: Early Modern Japan** (Cambridge: Cambridge University Press, 1991), xxviii + 831 pp.

Derek Massarella, **A World Elsewhere: Europe's Encounter with Japan in the Sixteenth and Seventeenth Centuries** (New Haven, Conn.: Yale University Press, 1990), xiii + 442 pp.

adaptations, of specific facets of the European and various Asian civilizations. Here, a sketch of what this story of interactive emergence might look like seems in order, to alert the reader to some of the main lines of change and interaction.

IN THIS STORY OF INTERACTIVE EMERGENCE, a first set of preconditions for the growth of commercial and cultural connections around the Indian Ocean was provided by the rise of Islam, which made the eastern Mediterranean and Persia an area of common religion and frequently of unified political authority,

prosperous and safe for merchants. Muslim traders who settled in the ports of India and Indonesia brought with them a unified set of commercial and legal practices that facilitated the growth of trade. The pilgrimage to Mecca added to the volume of maritime trade from India and Southeast Asia.

A second great set of contributions to trade was Chinese. China's "economic revolutions" after about 900 C.E. made it a source of large quantities of porcelain, silks, and other consumer goods that found markets throughout Eurasia. The superior ships and advanced commercial techniques of Chinese merchants facilitated the spread of Chinese shipping and enterprise across maritime Southeast Asia and into the Indian Ocean and contributed to the growth of indigenous trade-oriented economies and polities in many areas.<sup>2</sup>

Finally, the fine cotton and silk fabrics of the Indian subcontinent found markets everywhere and established especially enduring and structurally important linkages in two directions, to Island Southeast Asia and to the world of Islam, from Persia to the Arab lands to the Swahili ports of East Africa.

The political and cultural dimensions of this picture of interactive emergence are intriguing. The role of Islam in maintaining connections and spreading common practices has already been mentioned. The Portuguese carried the Christian war on Islam into the Indian Ocean, and that conflict eventually spread all the way to the Spice Islands and Mindanao. Asian Christian communities emerged from the interactions of European and Asian religious, cultural, and social idioms.

From the beginning, Indians did not just tolerate the Europeans; they employed them, rewarded them with revenue rights, and sought their protection in trade, in the process teaching them how to exploit the immense talents of the Indian peoples for production, commerce, and warfare. Thus European participation in Indian politics and political economy, not just profits of trade, sustained the European presence in the Indian Ocean from the sixteenth century on. Until about 1750, the Europeans had demonstrated distinctive "statist" modes of cohesion in simply surviving in the Indian Ocean and in articulating unusually wide systems of commercial coordination. In the late eighteenth and early nineteenth centuries, the British in Bengal combined these advantages with their own adeptness in exploiting Indian forms of political economy to outpace their Indian rivals, who also sought to build more effective mobilizing states. As a result, India became the prime fiscal and military base for the spread of British power in Asia.

Island Southeast Asia, the original goal of European voyages in search of the sources of spices, did not turn out to have the open-ended potential for empire building that was found in India, but its fractious small polities offered opportunities for meddling and domination that the Europeans found irresistible, and efforts to make territorial dominion profitable led to a series of important experiments in exploitative plantation monoculture.

China was as attractive to the Europeans as India for fine consumer goods, but China's centralized bureaucratic efforts to limit interaction with foreigners

<sup>2</sup> Mark Elvin, *The Pattern of the Chinese Past* (Stanford, Calif., 1973); William H. McNeill, *The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000* (Chicago, 1982), chap. 2.



presented a very different mode of what I have called interactive emergence. Political order and mercantile sophistication made possible large-scale trade with almost no foreign presence outside a few closely controlled ports. At least as important for the long-run evolution of maritime Asia was the anomalous situation of the maritime Chinese. The Chinese state almost never intervened in support of its maritime traders and emigrants. Commercially dominant but politically very much on their own, the overseas Chinese frequently became adept commercial and political allies of local rulers, whether Asian or European. Such key nodes of commerce and power in maritime Southeast Asia as Manila and Batavia were fundamentally dependent on Chinese productive, commercial, and organizational skills.

Even stranger were the consequences of dramatic political and social changes in Japan, which eventually led to complete prohibition of Japanese sea voyages. An early modern maritime Asia in which Japan projected beyond its shores its economic and military power would have had patterns of trade and power strikingly different from those that did develop.

The articulation of this picture of interactive emergence requires the comparison of several aspects of the cultures, economies, and politics of maritime Europe, the world of Islam, India, Southeast Asia, China, and Japan. These comparisons can contribute to many forms of history and also help to explain how it was that, of all these sophisticated and dynamic civilizations, the Europeans emerged in a position of steadily increasing dominance in Asian waters after about 1750. This field has implications for a variety of modes of thinking about the modern world as well as for the theory of the modern world-system.

UNTIL WORLD WAR II, scholarship in the history of European expansion in Asia was dominated by the European imperial powers, and many of the scholars and their readers were officials or former officials of the European empires. The postwar collapse of these empires led to a waning interest in and volume of scholarship in the European countries. A small number of scholars, most notably J. H. Parry and the remarkable C. R. Boxer, continued to make major contributions to the study of early European expansion. Eventually, a new generation of scholars developed who defined themselves as historians of one part of Asia or another, not of European expansion, and who frequently were themselves natives of the part of Asia they studied. These scholars use Asian-language sources, are interested in the internal dynamics of the various Asian civilizations, and read European-language sources for information about the Asian scene, not just the Europeans in it. This indigenization makes it much harder to keep up with the historiography and maintain a rounded picture of the subject. Historians of India, of Indonesia, and of China do not ask the same questions, do not read the same journals. I think it fair to assume that many of the authors of the recent books listed at the head of this essay and mentioned in its notes would not be aware of all the other books mentioned.

Scholars wanting to enter this field will find that there is a substantial

multinational network of communication. The Centre for the History of European Expansion at Leiden has been especially active in promoting conferences and interchanges of information, and its lively, solid journal *Itinerario* is the best single medium for keeping up with developments. Another center of current scholarship has formed in Paris.<sup>3</sup> By far the most energetic center of activity outside Europe is at the University of Delhi; some of the most interesting articles by scholars there and elsewhere in India appear in the *Indian Economic and Social History Review*.

The rich "European expansion" summaries of Parry, Boxer, and others are still indispensable reading for every student of this field.<sup>4</sup> It is regrettable that there has been more good summary writing on the sixteenth and seventeenth centuries than on the eighteenth, when some of the long-run consequences of the enduring European presence in Asian waters became dramatically apparent. This makes all the more welcome the excellent eighteenth-century material in Holden Furber's fine survey of the East India Companies and the rich and interesting works on this era by Parry and by Glyndwr Williams.<sup>5</sup>

Among the books listed at the head of this essay, the two distinguished conference volumes edited by James Tracy offer the most sophisticated and multidimensional access to what is going on in this area. *The Rise of Merchant Empires* includes several essays focused on Asian commercial history, some good examples of quantifying economic history relevant to European trade in the Indian Ocean, and a series of surveys of the overseas trade of European countries that vary considerably in their attention to trade with Asia. Tracy's introduction gives an intelligent summary of some key issues. With the more recent *Political Economy of Merchant Empires*, several key methodological issues come nicely into focus. What were the relative contributions, on the one hand, of new modes of organization of force and articulation of power and commerce and, on the other, of reductions of transport and other costs to the long drift to European dominance over world trade? Russell Menard's carefully argued essay on transport costs finds the evidence for major European reduction or advantage in this important and often-discussed factor uneven for most lines of maritime trade and weak for European-Asian trade. On the political economy side, M. N. Pearson's "Merchants and States" opens up key comparative dimensions; I will return to it

<sup>3</sup> The journal of this group is *Moyen Orient et Océan Indien*, continuing the earlier *Mare Luso-Indicum*. A fine example of this school of scholarship is Geneviève Bouchon, "Notes on the Opium Trade in Asia during the Pre-Colonial Period," in Roderick Ptak and Dietmar Rothermund, *Emporia, Commodities and Entrepreneurs in Asian Maritime Trade, c. 1400-1750* (Stuttgart, 1991), 95-106.

<sup>4</sup> C. R. Boxer, *The Dutch Seaborne Empire, 1600-1800* (New York, 1965); Boxer, *The Portuguese Seaborne Empire, 1415-1825* (New York, 1969); J. H. Parry, *The Age of Reconnaissance* (London, 1963); Parry, *The Spanish Seaborne Empire* (New York, 1966); Parry, *The Discovery of the Sea* (Berkeley, Calif., 1981); Bailey W. Diffie and George D. Winius, *Foundations of the Portuguese Empire, 1415-1580* (Minneapolis, Minn., 1977); G. V. Scammell, *The World Encompassed: The First European Maritime Empires, c. 800-1650* (Berkeley, 1981); Scammell, *The First Imperial Age: European Overseas Expansion c. 1400-1715* (Boston, 1989). Boxer's immense productivity can be seen in S. George West, *A Complete Bibliography of the Works of C. R. Boxer, 1926-1983* (London, 1984), which will have to be supplemented someday for Boxer's publications in recent years.

<sup>5</sup> Holden Furber, *Rival Empires of Trade in the Orient, 1600-1800* (Minneapolis, Minn., 1976); J. H. Parry, *Trade and Dominion: The European Overseas Empires in the Eighteenth Century* (New York, 1971); Glyndwr Williams, *The Expansion of Europe in the Eighteenth Century* (New York, 1967).

below in connection with his other work. Tracy's introduction again does an admirable job of laying out these issues and adds some especially telling detail on Southeast Asian examples of maritime profit and power mobilization.

Two other works may be noted as fine examples of the contributions to Asian maritime history that can be made by works focused primarily on Europe. Jonathan Israel's *Dutch Primacy in World Trade* gives full coverage to the rise of Dutch power in Asia, with an excellent grasp of fact and literature. Israel makes two useful contributions to understanding the Dutch in Asia. First, by arguing that Dutch trade hegemony in Europe was advanced at several crucial points by state naval power, he makes the precocious "statist" centralization of the Dutch East India Company seem less anomalous than did the older view of an almost stateless Dutch dominance in Europe and the Atlantic. Second, his emphasis on the economic functions and power of Amsterdam as an entrepôt for all kinds of goods, especially luxury goods, from all parts of the world reinforces the emphasis on entrepôts in Asian maritime history and gives more importance to Asian trade as a source of Dutch domination of European trade than did the earlier emphases on quantities of goods imported and money earned.

Geoffrey Parker's *Military Revolution*, parts of which are summarized in his essay in Tracy's *Political Economy*, begins with an expert summary of changes in military organization and technology in early modern Europe and then in its last two chapters turns to the roots of Europe's growing ability to project its power around the world.<sup>6</sup> Parker draws on a remarkable range of relevant scholarship in Asian and African history to compare the military, technological, and organizational goals and capacities of the Asian and African peoples to those of the Europeans and to isolate the multiple and contingent origins of European naval and military superiority. This is an elegant example of the way in which comparative thinking can yield insight into the emergence of a world-historical trend.

The issues of the comparative history of political economy and of state building opened up by the Tracy volumes and by Parker are developed with some fascinating twists in Bayly's *Imperial Meridian*. This book begins with a sweeping account of parallel economic, political, and cultural changes in the great Muslim empires during the late eighteenth and early nineteenth centuries. Then it turns back to changes in relations among England, Scotland, and Ireland and the projections of British power across the Atlantic and in Europe. But India and the world of Islam repeatedly come into focus. The increasing effectiveness of organization and will to control in India and other parts of the British empire is seen as analogous to the state-building efforts of regional hegemonies in the Islamic empires. Resistance to indigenous and colonial power and Islamic revival march alongside the ideas and stimulus of the French Revolution as sources of popular turmoil around the world. This book will be a profoundly de-centering experience for many historians, especially those of the French Revolution and of the "Early National" period in the United States.

<sup>6</sup> This theme has attracted the attention of a number of other eminent historians. See McNeill, *Pursuit of Power*, chaps. 3–5; Carlo M. Cipolla, *Guns, Sails, and Empires: Technological Innovation and the Early Phases of European Expansion, 1400–1700* (New York, 1965), chap. 2; and Parry, *Age of Reconnaissance*, chap. 7.

Bayly's *Indian Society and the Making of the British Empire* presents a more detailed picture of efforts in several regions of India to build up state structures that would extract larger resources of wealth and military manpower from their subject populations than the Mughal empire, for all its effective central control, had wanted or needed to. Bayly interprets these efforts not just as results of group and regional conflicts that intensified as Mughal control declined but also as responses to the growth and specialization of commerce and agriculture that made greater resources available. The English in India emerge as one of these regional state-building powers, competing with and learning from the others, given an edge by their external resources but still finding some of the other state builders formidable adversaries. This is brilliant and provocative comparative history of politics and political economies, and it makes crucial contributions to an indigenized interpretation of the drift to European maritime hegemony in the Indian Ocean.

Other works listed offer abundant evidence both of the richness of facts, topics, and sources in Asian maritime history and of the uneven state of development and the difficulty of keeping it all in focus. The volume edited by Ptak and Rothermund contains revised versions of papers presented at a 1989 conference, which was a rare chance for sustained interaction between historians of the Indian Ocean and those who study the trade around the South China Sea. Some of the papers are highly specialized, especially where Sinological and Indological techniques are applied to the study of specific commodities. Those studied include such surprises as opium, salt, tortoise shell, deerskins, and bird nests, as well as the better-known spices and textiles. Entrepreneurs studied include a Persian, a Chinese, and various Indians, as well as Europeans.

K. N. Chaudhuri has given us three works of broad coverage and synthesis. *The Trading World of Asia and the English East India Company, 1660–1760*<sup>7</sup> is one of the finest works in this field. *Trade and Civilisation in the Indian Ocean*<sup>8</sup> provides some excellent Asia-centered material on the rise of the Islamic and Chinese spheres of production and trade, on commodities, shipping, and entrepôts in Asian trade, and on the European intrusions. Chaudhuri's more recent *Asia before Europe* will be welcomed by every student of the field for its richness of reading and ambition in interpretation, especially in dealing with the Islamic world, but it is not particularly concerned with the world of maritime trade. Chaudhuri is far better read and more at home in Asian history than Fernand Braudel ever was, but to this reader his wide range of theoretical reading and reference sometimes induces even more bafflement than Braudel's *pointillisme*.<sup>9</sup> Braudel's influence can be seen in an emphasis on the *longue durée* that obscures some substantial changes in economic and commercial patterns and in a related lack of interest in states and political economies.

The conference volume edited by Denys Lombard and Jean Aubin seeks to redress excessive emphasis on the Europeans in the Indian Ocean by focusing

<sup>7</sup> Cambridge, 1978.

<sup>8</sup> K. N. Chaudhuri, *Trade and Civilisation in the Indian Ocean: An Economic History from the Rise of Islam to 1750* (Cambridge, 1985).

<sup>9</sup> Fernand Braudel, *Civilization and Capitalism: 15th–18th Century*, 3 vols. (London, 1979, 1982, 1984).

almost entirely on Asian merchant communities. Specific topics range over eight centuries and from East Africa to Japan. Some of the studies are primarily summaries of earlier scholarship or addenda to works the authors have already published elsewhere. Others are rich in citations of primary sources in Asian languages. For the topic of the present essay, this collection is especially valuable as a guide to the facts and literature on the Muslim trading communities around the Indian Ocean.

M. N. Pearson has made a number of important contributions to the study of political economy and state building. His *Before Colonialism* is a rare effort at summary and methodological self-consciousness about one set of issues, those raised by Wallerstein. Concentrating on the inconsistencies and inadequacies of Wallerstein's comments about the Indian Ocean, however, Pearson overlooks a central theoretical difficulty. Wallerstein's world-system paradigm has no place for developments that have not yet led to the economic peripheralization of an area within the modern capitalist world economy. The background to economic peripheralization is sought primarily in structures of economic dominance and exploitation. But in maritime Asia, the European presence took a number of novel statist forms, first the ramshackle Estado da India, then the great monopolistic companies, which maintained a European presence for over 250 years until the climactic state-building competition in late eighteenth-century India led to full-scale peripheralization in the world-system.

Pearson's work *The Portuguese in India* can be read as moving toward the comparative history of state building, for which the amazing things the Portuguese wanted to do in dominating Asian trade are as important as their failures to reach their goals. But it is in his essay "Merchants and States" in Tracy's *Political Economy* that Pearson makes his most important contribution to the necessary comparative themes. A distinction between "despotic" and interventionist states leads eventually to the generalization that Asian states, more or less in the despotic mold, might have been hospitable to commerce but almost never actively promoted it. The Estado da India and the companies are seen as intrusions of the European interventionist, trade-promoting type of state building into maritime Asia. Much remains to be done in fleshing out the implications of these views and making the necessary qualifications of them. Fuller use still can be made of the intriguing work of Niels Steensgaard, who distinguished the "redistributive" nature of the Portuguese Estado da India from the efforts of the companies to internalize protection costs and drive their competitors out of the market.<sup>10</sup> Bayly's picture of the shift to the building of resource-mobilizing territorial states also is of fundamental importance. Tracy's introduction and Thomas Brady's essay, "The Rise of Merchant Empires, 1400–1700: A European Counterpoint" in *Political Economy*, offer valuable supplements to this discourse on empires and modes of state building. None of these authors has made use of the considerable increase of interest in states and state building seen in several social sciences in recent years.<sup>11</sup>

<sup>10</sup> Niels Steensgaard, *The Asian Trade Revolution of the Seventeenth Century: The East India Companies and the Decline of the Caravan Trade* (Chicago, 1974).

<sup>11</sup> For starting points, see Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, eds.,



BAYLY CALLS EARLY MODERN MARITIME HISTORY one of the best-researched fields in Indian history, a claim that could not conceivably be made for the maritime history of any other part of Asia. The predominance of books about India among the works discussed in this essay is striking but not surprising. For anyone working in any part of the history of maritime Asia, these works are indispensable reading, both because of the variety of topics and approaches they open up and because India's maritime connections affected every other Asian coastline. Some recent surveys will help us get our bearings. There are excellent chapters on maritime trade in Volumes 1 and 2 of the *Cambridge Economic History of India*.<sup>12</sup> Das Gupta and Pearson's *India and the Indian Ocean* is a useful guide to the questions and literature of the field, although it inevitably suffers from the overlaps and inconsistencies of an edited volume.

For the sixteenth century, there are some excellent studies of the Portuguese and their Indian allies and rivals, heavily dependent on Portuguese sources. The Portuguese never had the human or fiscal resources to drive their Muslim rivals from the seas. How, then, did they survive and in some cases prosper in the Indian Ocean? First, the Indian commercial economy was so large and energetic that many of its merchants and almost all Indian rulers found it cost-effective to buy a sea pass (*cartaz*) from the Portuguese or simply bribe their customs officials charged with excluding the Muslims from certain lines of trade. Second, the Portuguese presence in India soon came to derive most of its economic vitality not from trade between India and Europe but from participation in trade within Asia, much of it in close cooperation with Asian merchants. Thus efforts to understand the dynamics of the Portuguese presence lead to an "indigenizing" attention to their interaction with Asian economies. And while the Portuguese sources are unsystematic and hard to use, they tell us a great many things we can learn from no Asian-language source. In recent Portuguese scholarship on these topics, the contributions of V. M. Godinho loom very large; the four volumes of his *Descobrimentos e a Economia Mundial*<sup>13</sup> are a wonderful collection, very much in the *Annales* style, of fresh data, compilations of figures from many sources, and suggestive interpretations of many issues. Organized projects on the Portuguese in Asia have languished in Portugal since the end of the empire after the 1974 revolution, even as the old restraints of imperial ideology evaporated. One remaining forum has been a series of international conferences and their published papers.<sup>14</sup> In India, there has

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*Bringing the State Back In* (Cambridge, 1985); Charles Tilly, *Coercion, Capital, and European States, A.D. 990–1990* (Oxford, 1990); and Jack A. Goldstone, *Revolution and Rebellion in the Early Modern World* (Berkeley, Calif., 1991).

<sup>12</sup> K. N. Chaudhuri, "European Trade with India," and A. Dasgupta, "Indian Merchants and the Trade in the Indian Ocean," in Tapan Raychaudhuri and Irfan Habib, eds., *The Cambridge Economic History of India*, Volume 1 (Cambridge, 1982), 382–433; Chaudhuri, "Foreign Trade and the Balance of Payments (1757–1947)," in Dharma Kumar, ed., *The Cambridge Economic History of India*, Volume 2 (Cambridge, 1983), 804–77.

<sup>13</sup> 2d edn. (Lisbon, 1981–83); this is fuller and more up-to-date than the first Portuguese edition or the French version, *L'économie de l'empire portugais aux XV<sup>e</sup> et XVI<sup>e</sup> siècles* (Paris, 1969).

<sup>14</sup> See, for example, Luís de Albuquerque and Inácio Guerreiro, eds., *Actas do II Seminário Internacional de História Indo-Portuguesa* (Lisbon, 1985).

been an important indigenized revival of scholarship at and concerning Goa.<sup>15</sup>

Pearson's study *The Portuguese in India*, a most useful survey with an excellent bibliography, maintains a focus on Portuguese institutions and actions, less strongly indigenizing in thrust than his earlier work on the Portuguese in Gujarat<sup>16</sup> but still rich in openings for Portuguese-Indian and Portuguese-Muslim comparisons. Everywhere from Mozambique to Island Southeast Asia, the Portuguese encountered well-established and sophisticated Muslim merchant communities and followed Muslim trade routes.<sup>17</sup> The gratuitous and indiscriminate violence of Portuguese assaults in their first phase frequently was successful, despite the overwhelming numerical superiority of the Muslims, because Muslim ships in the Indian Ocean, unlike those in the Mediterranean, rarely carried arms and used ships that were ill suited to the mounting and firing of cannon. The Ottoman empire and the Mamluk state in Egypt faced many challenges internally and on their other frontiers and had to mount their Indian Ocean expeditions from desert coasts; they were only occasionally able to send a major fleet to defend their fellow Muslims in India against Portuguese attacks and did not establish a permanent base or presence on the Indian coast. The Portuguese Estado da India was a ramshackle and conflict-ridden structure, but the Muslims in the Indian Ocean had *no* overarching political structure of their own. This lack seems to have facilitated Portuguese efforts to exploit local differences and find Muslim allies.<sup>18</sup> An early, short-lived exception to the general weakness of Muslim mobilization was the effort of a powerful group on the Malabar (southwest) coast to mobilize naval power and mercantile wealth to resist the Portuguese.<sup>19</sup> A later and more dramatic exception was the naval power of Oman, which rose to dominate the Persian Gulf after the Persians and the English expelled the Portuguese from Ormuz, then extended its power and commerce down the east coast of Africa, building warships in Indian ports, forcing the Portuguese from their very thinly held strong point at Mombasa.<sup>20</sup> At the mouth of the Red Sea, Portuguese attacks

<sup>15</sup> Teotonio R. De Souza, *Medieval Goa: A Socio-Economic History* (New Delhi, 1979); De Souza, ed., *Indo-Portuguese History: Old Issues, New Questions* (New Delhi, 1985); De Souza, ed., *Essays in Goan History* (New Delhi, 1989).

<sup>16</sup> M. N. Pearson, *Merchants and Rulers in Gujarat: The Response to the Portuguese in the Sixteenth Century* (Berkeley, Calif., 1976).

<sup>17</sup> R. B. Serjeant, "Yemeni Merchants and Trade in Yemen, 13th–16th Centuries"; J. Aubin, "Marchands de Mer Rouge et du Golfe Persique au tournant des 15<sup>e</sup> et 16<sup>e</sup> siècles"; J. Calmard, "Les marchands iraniens: Formation et montée d'un groupe de pression, 16<sup>e</sup>–19<sup>e</sup> siècles"; and Serjeant, "The Hadrami Network," all in Denys Lombard and Jean Aubin, eds., *Marchands et hommes d'affaires asiatiques dan l'Océan Indien et la Mer de Chine 13<sup>e</sup>–20<sup>e</sup> siècles* (Paris, 1988), 61–107, 147–54.

<sup>18</sup> Diffie and Winius, *Foundations of the Portuguese Empire*, chap. 15; George W. F. Stripling, *The Ottoman Turks and the Arabs, 1511–1574* (Urbana, Ill., 1942).

<sup>19</sup> Geneviève Bouchon, "Regent of the Sea": *Cannanore's Response to Portuguese Expansion, 1507–1528*, Louise Shackley, trans. (Delhi, 1988); see also the summary in Bouchon, "Sixteenth Century Malabar and the Indian Ocean," in Ashin Das Gupta and M. N. Pearson, eds., *India and the Indian Ocean, 1500–1800* (Calcutta, 1987), 162–84, esp. 169, 174–75, 181.

<sup>20</sup> R. D. Bathurst, "Maritime Trade and Imamate Government: Two Principal Themes in the History of Oman," in Derek Hopwood, ed., *The Arabian Peninsula: Society and Politics* (Totowa, N.J., 1972), 89–106; C. R. Boxer and Carlos de Azevedo, *Fort Jesus and the Portuguese in Mombasa, 1593–1729* (London, 1960); Edward A. Alpers, *Ivory and Slaves: Changing Pattern of International Trade in East Central Africa to the Later Nineteenth Century* (Berkeley, Calif., 1975), 62.

were driven off, and the later Dutch and English coffee trade in Yemen was completely dependent on the toleration of the local authorities.<sup>21</sup>

Pearson, Parker, and others have pointed out the apparent cost-effectiveness of Indian acquiescence in Portuguese extortion. But historians accustomed to the coastal defenses and commercial rivalries of early modern Europe or to the vigilance against foreign intrusion of early modern China and Japan will find the Indian tolerance of the Portuguese presence and failure to mobilize a sustained counterattack amazing. A full explanation of these non-responses will have to pay some attention to the nature of Indian society, in which divisions into endogamous occupational groups (the "caste system") and the intricacies and tensions of Hindu-Muslim relations made broad territorial solidarities, or even unified fronts in a single port city, very hard to achieve. Bayly shows us that mobilizing territorial states did emerge in India, but in the late eighteenth century, not the late sixteenth. It also is important to remember the particular historical situation in the sixteenth century. The Hindu empire of Vijayanagar collapsed. The Muslim sultanates steadily lost ground to the Mughal advance from the north. Afonso de Albuquerque (1453–1515) struck right along the seam of this political division in conquering and holding Goa; later, the rise of the Maratha-Mughal conflict in Goa's back yard guaranteed its survival.

Another important account of the Portuguese presence, complementary to Pearson's in many ways, is Sanjay Subrahmanyam and Luís Filipe F. R. Thomaz's "Evolution of Empire: The Portuguese in the Indian Ocean during the Sixteenth Century" in Tracy's *Political Economy*. In addition to many fresh views on shifts in the Portuguese goals and concepts of empire, this excellent essay reflects the authors' work on Indian trade and Portuguese involvement in it on the Coromandel (southeast) coast and around the Bay of Bengal. In his own works, especially *The Political Economy of Commerce*, Subrahmanyam provides some of the most sharply argued positions of an indigenized historiography of the Indian Ocean. He criticizes the tendency of previous authors to portray the Indian economy as largely pre-market, changing only very slowly, except for a limited sphere of production of and trade in luxury goods. Subrahmanyam builds up a complex picture of dynamic economies in which basic foodstuffs were traded widely by land and by sea, many people depended for their livelihoods on agricultural and handicraft production for distant markets, and rich merchants and political power-holders sought to secure their positions and minimize their risks by becoming "portfolio capitalists" who invested in a wide variety of handicraft, commercial, and tax-farming operations and in political influence at one or more regional courts. Portuguese participation in this economy, Subrahmanyam shows, was constantly changing and complexly symbiotic, involving cooperation with some local producers, merchants, and "portfolio capitalists" and conflict with others. The general pattern is described as one of "contained conflict," in which the effects of Portuguese maritime dominance and violence were blunted and countered by Indian wealth, military manpower, and commercial and political acumen.

<sup>21</sup> C. J. Brouwer, *Cauwa ende Comptanten: De Vereenigde Oostindische Compagnie in Jemen/Cowha and Cash: The Dutch East India Company in Yemen* (Amsterdam, 1988); Chaudhuri, *Trading World*, chap. 16.

Thus Subrahmanyam demonstrates that the Portuguese already were involved in the first stages of a pattern of interactive emergence of European power in Asia, learning from and sharing in distinctive Indian practices of trade and of revenue collection. He mentions several cases of Indian rulers granting revenue collection rights to local Portuguese and even to a local branch of the Society of Jesus.<sup>22</sup> At Goa, as Pearson and others have shown, the Portuguese maintained almost intact the land-holding and revenue collection system of their Indian predecessors.<sup>23</sup> In Bengal, there is evidence for several grants by local sovereigns and Mughal emperors to the Portuguese of rights to collect local land taxes and customs dues.<sup>24</sup>

India was and is an immense and self-conscious field of experiment in relations among various groups defined by status, occupation, religion, and ethnic heritage, and of confrontation, debate, and accommodation among religious and philosophical teachings. The Portuguese and their Roman Catholicism were new elements in this mix. At Goa and a few other centers, Catholicism brought forms of authority and institutional continuity that were new to India. These had considerable effect: episcopal hierarchy, religious orders (especially the Jesuits), Holy Inquisition, and municipal governments. Pearson has two good chapters on the Goa situation. Boxer has made many contributions to this kind of social-cultural history, including a book showing the common patterns of municipal organization throughout the Portuguese empire.<sup>25</sup> Teotonio de Souza and his colleagues have written about Goa with the special insight of erudite insiders. Subrahmanyam tells us a great deal about Cochin on the Malabar coast and the various Coromandel settlements. Despite all this good work, until recently it was hard to see the full range and challenge of studies of Christianity in India, which require the contributions both of the ethnohistorian of intergroup relations and of the student of the confrontations of text-based high cultures. Now the comprehensive history by Stephen Neill provides excellent guidance, sympathetic both to Indian culture and to Christianity.<sup>26</sup> The most intriguing effort to open a dialogue with the textually based high traditions of Hinduism, that of the Jesuit Robert de Nobili, has had several good narrative accounts, including a chapter in Neill, but only recently has a Jesuit scholar of Hinduism, Francis X. Clooney, begun to show how Nobili adapted specific themes of the South Indian Vaisnava Hinduism of his time.<sup>27</sup> The breakthrough in studies of Christian interactions

<sup>22</sup> Sanjay Subrahmanyam, *Improvising Empire: Portuguese Trade and Settlement in the Bay of Bengal 1500–1700* (Delhi, 1990), 67, 74, 121.

<sup>23</sup> M. N. Pearson, "Indigenous Dominance in a Colonial Economy: The Goa *Rendas*, 1600–1670," *Mare Luso-Indicum*, 2 (1972): 61–73; de Souza, *Medieval Goa*, chaps. 2, 3.

<sup>24</sup> J. J. A. Campos, *History of the Portuguese in Bengal* (1919; rpt. edn., New York, 1975), chaps. 4, 5.

<sup>25</sup> C. R. Boxer, *Portuguese Society in the Tropics: The Municipal Councils of Goa, Macao, Bahia, and Luanda, 1510–1800* (Madison, Wis., 1965); Boxer, *Race Relations in the Portuguese Colonial Empire, 1415–1825* (Oxford, 1963); Boxer, *The Church Militant in Iberian Expansion, 1440–1770* (Baltimore, Md., 1978).

<sup>26</sup> Stephen Neill, *A History of Christianity in India: The Beginnings to AD 1707* (Cambridge, 1984); Neill, *A History of Christianity in India, 1707–1858* (Cambridge, 1985).

<sup>27</sup> Francis X. Clooney, S.J., "Christ as the Divine Guru in the Theology of Robert de Nobili," in Ruy Costa, ed., *One Faith, Many Cultures* (Maryknoll, N.Y., 1988), 25–40; Clooney, "Roberto de Nobili, Adaptation, and the Reasonable Interpretation of Religion," *Missiology*, 18 (1990): 25–36.

with Indian culture is seen in Susan Bayly's book *Saints, Goddesses, and Kings*. Drawing on wide reading in sources and secondary literature and on field work in southern India, Bayly shows how Muslims, the Syrian-rite "Thomas Christians," whose roots go back long before Vasco da Gama, and more recent Roman Catholic converts all adapted Indian idioms of community and caste organization (including vehement "honors disputes" among themselves), patron deities and founding holy men, and pilgrimage to sites associated with them. This is a stunning example of the kind of richly textured picture of cultural interactive emergence that can be drawn out of a combination of ethnographic and historical approaches to the history of Christianity in Asia.

FOR ANY INDIGENIZED OR EUROCENTRIC ACCOUNT of the history of the Indian Ocean, the arrival of the Dutch and the English at the end of the sixteenth century marks a turning point. The organizational cohesion of the Dutch and English East India Companies far surpassed that of the Portuguese. Their military and naval power generally was subordinated to commercial objectives, although these might tacitly include the private commercial interests of the servants of the companies. Thus they form a remarkable early chapter in the history of bourgeois statism, of power in the service of profit. The wonderful record-keeping of the companies (especially the Dutch Company) makes possible some of the best research we have on the politics and commerce of the Indian Ocean. Subrahmanyam's studies, and those of many other scholars, of the Portuguese after 1600 owe a great deal to Dutch sources. The most important single demonstration of what can be accomplished in studies of the companies in their Asian contexts is Chaudhuri's massive volume *Trading World of Asia*, full of sophisticated and closely reasoned analysis of aspects of the Indian commercial economy *per se* as well as of the English East India Company and its activities in India. Several others have made excellent use of the Dutch sources.<sup>28</sup> Das Gupta's study of Surat is especially noteworthy for his description of the immense web of private trade connections between Indian merchants and employees of the Dutch and English East India Companies. In all these books, the authors' deep knowledge of Indian social and economic history combines with the specifics noted in company records to make possible very detailed discussions of the social organization of production, especially in the great textile industries;<sup>29</sup> of the relations of producers and merchants with various levels of political power; of currency, credit, and prices; and much more. These books ought to provide food for thought to all students of early modern comparative economic history.

<sup>28</sup> Ashin Das Gupta, *Indian Merchants and the Decline of Surat: C. 1700–1750* (Wiesbaden, 1979); Om Prakash, *The Dutch East India Company and the Economy of Bengal, 1630–1720* (Princeton, N.J., 1985); Tapan Rayachaudhuri, *Jan Company in Coromandel, 1605–1690* (The Hague, 1962); S. Arasaratnam, *Merchants, Companies and Commerce on the Coromandel Coast, 1650–1740* (Delhi, 1986).

<sup>29</sup> Chaudhuri, *Trading World*, chaps. 11, 12; Joseph J. Brenig, "Textile Producers and Production in Late Seventeenth Century Coromandel"; and S. Arasaratnam, "Weavers, Merchants, and Company: The Handloom Industry in Southeastern India 1750–1790," in Subrahmanyam, *Merchants, Markets, and the State*, 66–89, 190–214.



During the eighteenth century, the Dutch Company became increasingly committed to territorial control in Indonesia. In India, the great areas of trade growth, Bengal and Coromandel, were not controlled by any one European power and could be brought under such control only through a long process of deepening involvement in the internal politics of India. The English Company, less burdened by military commitments elsewhere, also was considerably more tolerant than the Dutch of private trade in its ports by Indian merchants, Europeans not in its service, and even its own employees. British private traders became adept exploiters of commercial connections and small margins of legal and semi-legal privilege.<sup>30</sup> The wealth and experience of these private traders and the trade they and their Indian partners brought to English ports—products of their own assimilation into the complexities and riches of the Indian economy—were sources of English strength (and sometimes of precious records for the economic historian). The growing scale and frequency of war in India as the Mughal power declined and formidable regional mobilizing states emerged presented amazing opportunities for French and British adventurers to gain fortunes in treasure and revenue rights by training and leading the armies of Indian princes. After 1757, the English Company became the supreme power in Bengal. This was a process of world-historical importance, the culmination of the interactive emergence of European power in maritime Asia. Indian treasure and Indian opium propelled the expansion of the British tea trade to China. British private traders from India sold large quantities of guns and opium throughout Southeast Asia. These developments have attracted many researchers, but until recently they had not received an up-to-date summary. Now there are two, nicely complementary, in Bayly's *Indian Society* and Marshall's *Bengal: The British Bridgehead*. Both are sophisticated and rich in analyses of Indian changes and contexts. Neither does very much with the remarkable personalities and strange events so often described—Clive, Hastings, Cornwallis, the Black Hole, and others. For these and for the free-wheeling, cynical, corrupt, and immensely dynamic flavor of the times, one will have to turn to earlier general histories.<sup>31</sup> Hameeda Hossain's *Company Weavers* is an excellent example of the kind of detailed history of commerce and production that can be written from the English records, and it paints a melancholy picture of once-stable weaver communities driven to destitution by British pressures for production and revenue.

For the rest of early modern maritime Asia, the current state of scholarship is much less even, and the shift from Eurocentrism to indigenization and interactive emergence less decisive. Ceylon or Sri Lanka, geographically associated with the Indian subcontinent, stood somewhat apart both in its predominant Buddhist culture and in its commercial economy; as the early modern world's most important producer of cinnamon (for which the leading markets were in Spain and Spanish America<sup>32</sup>), it was a tempting target for European monopoly

<sup>30</sup> Furber, *Rival Empires of Trade*, chap. 6; Ian Bruce Watson, *Foundations for Empire: English Private Trade in India, 1659–1760* (New Delhi, 1980).

<sup>31</sup> Furber, *Rival Empires of Trade*, chap. 3; Parry, *Trade and Dominion*, chap. 9; Williams, *Expansion of Europe*, chaps. 5, 6.

<sup>32</sup> Jonathan I. Israel, *Dutch Primacy in World Trade, 1585–1740* (Oxford, 1990), 316.

enforcement strategies characteristic of all spice-producing areas. Modern studies of Sri Lanka have been fullest on its political relations with Europeans.<sup>33</sup> Much remains to be done on the Roman Catholic missions and their widespread cultural impact, but there is a recent solid contribution on the Dutch missionary and educational effort.<sup>34</sup>

IN ACCOUNTS OF EUROPEAN EXPANSION in maritime Asia, Southeast Asia is seen as a mercantile monopolist's dream. Its ports were hospitable to foreign merchants, who lived under their own headmen and interacted with a highly commerce-minded local elite.<sup>35</sup> The centers of production of cloves and nutmeg could be brought under tight control and huge monopoly profits made on sales in Europe. Pepper was cheaper and more widely grown, much harder to monopolize, but most Southeast Asian port cities had rather limited hinterlands and thus were vulnerable to attack or blockade from the sea. Trade from the Spice Islands and almost all Indonesian ports to India, the Middle East, and Europe had to pass through one of two narrow, patrollable straits, Sunda and Malacca. The Europeans who grasped these potentialities were following the examples of centuries of indigenous power-holders, who had focused on controlling the two straits and establishing loose hegemony over scattered and vulnerable port cities. During the European intrusion, a number of Muslim polities oriented toward maritime trade, raiding, and power arose in Southeast Asia: Johore,<sup>36</sup> Achen,<sup>37</sup> Makassar,<sup>38</sup> Banda,<sup>39</sup> and Sulu.<sup>40</sup> Of these, Achen was by far the most effective in maintaining its own independence and its own trade network and in counterattacking the Europeans. But none of these states lasted as long or spread its net of trade and power as wide as the Dutch East India Company. Reid's admirable *annaliste* account of the enduring patterns of trade, settlement, and production in this island world is a model of what a student of indigenizing social science can learn from close reading of European sources. It does not advance very far toward patterns of empire and political economy; those will be the topics of a promised

<sup>33</sup> S. Arasaratnam, "Ceylon in the Indian Ocean Trade: 1500–1800," in Das Gupta and Pearson, *India and the Indian Ocean*, 224–39; Arasaratnam, *Dutch Power in Ceylon, 1658–1687* (Amsterdam, 1958); K. W. Goonewardena, *The Foundations of Dutch Power in Ceylon, 1638–1658* (Amsterdam, 1958); George D. Winius, *The Fatal History of Portuguese Ceylon* (Cambridge, Mass., 1971).

<sup>34</sup> J. van Goor, *Jan Kompenie as Schoolmaster: Dutch Education in Ceylon, 1690–1795* (Groningen, 1978).

<sup>35</sup> Denys Lombard, "Le sultanat malais comme modèle socio-économique," in Lombard and Aubin, *Marchands et hommes d'affaires asiatiques*, 117–24; M. A. P. Meilink-Roelofs, *Asian Trade and European Influence in the Indonesian Archipelago between 1500 and about 1630* (The Hague, 1962).

<sup>36</sup> Leonard Andaya, *The Kingdom of Johor, 1641–1728* (Kuala Lumpur, 1975).

<sup>37</sup> Denys Lombard, *Le Sultanat d'Atjeh au temps d'Iskandar Muda (1607–1636)* (Paris, 1967).

<sup>38</sup> Leonard Andaya, *The Heritage of Arung Palakka: A History of South Sulawesi (Celebes) in the Seventeenth Century* (The Hague, 1981).

<sup>39</sup> John Villiers, "Trade and Society in the Banda Islands in the Sixteenth Century," *Modern Asian Studies*, 15 (1981): 743–49; James D. Tracy, "Introduction," in Tracy, ed., *The Political Economy of Merchant Empires* (Cambridge, 1991), 3, 15; Willard A. Hanna, *Indonesian Banda: Colonialism and Its Aftermath in the Nutmeg Islands* (Philadelphia, 1978).

<sup>40</sup> James F. Warren, *The Sulu Zone, 1768–1898* (Singapore, 1981).

second volume.<sup>41</sup> The best recent political and cultural summary in English is that by M. C. Ricklefs.<sup>42</sup>

Historians have understood for many years that the Europeans first established themselves as non-privileged participants in the multiethnic life of the Southeast Asian ports and even when they came to wield ultimate power made use of indigenous structures and practices to control Asian traders and settlers. Recently, we have begun to see some additions to an indigenized account of the rise of Dutch power in Indonesia. The Chinese, already active in the area, became key allies of the Dutch, engaging in local trade, carrying a share of the trade between Java and China and among islands in Indonesia, contracting with the Dutch to collect various local taxes, and later, when rural areas near Batavia were sufficiently free of Javanese resistance, building up a thriving sugar industry. Leonard Blussé, whose excellent collection of articles, *Strange Company*, represents the best work yet done on these connections, has called Batavia a “Chinese colonial town.”<sup>43</sup> Many Dutch Company officials married highly capable part-Indonesian women who managed their private trade.<sup>44</sup> Even the progress of the Dutch toward wider dominion had roots in Indonesian political culture. In the highly personalized small states of Indonesia, inter-generational rivalry and unstable sharing of power and sovereignty were the norm; in such a situation, it was as normal for one rival or the other to call for Dutch assistance as it had been to call for the assistance of earlier indigenous maritime overlords.<sup>45</sup> A dramatic example of this was a remarkably brief campaign in 1682 in which the Dutch Company, called in by the Young King of Banten to aid him in a protracted conflict with his father the Old King, made Banten a client state, no longer a rival to Batavia as the entrepôt of west Java.<sup>46</sup> (The pattern has survived into our own time; the sultan of Brunei carried on a long struggle in the 1970s and 1980s with his father, who had abdicated.<sup>47</sup>)

In the larger arena of Javanese politics, the Dutch are shown being drawn farther and farther in by the unstable and highly personalized politics of the Javanese courts<sup>48</sup> and then obtaining the cooperation of Javanese rulers in some highly exploitative systems of forced-delivery monoculture. The first great successes were in the production of coffee, making possible the continued

<sup>41</sup> See also Anthony Reid, “An ‘Age of Commerce’ in Southeast Asia,” *Modern Asian Studies*, 24 (1990): 1–30; and Reid, “The Seventeenth-Century Crisis in Southeast Asia,” *Modern Asian Studies*, 24 (1990): 639–59.

<sup>42</sup> M. C. Ricklefs, *A History of Modern Indonesia, c. 1300 to the Present* (Bloomington, Ind., 1981), esp. chaps. 3–9. See also Kenneth R. Hall, *Maritime Trade and State Development in Early Southeast Asia* (Honolulu, 1985), chaps. 8, 9.

<sup>43</sup> Another important recent contribution is Denys Lombard, *Le carrefour javanais: Essai d'histoire globale*, 3 vols. (Paris, 1990), vol. 2, chap. 4.

<sup>44</sup> Jean Gelman Taylor, *The Social World of Batavia: European and Eurasian in Dutch Asia* (Madison, Wis., 1983).

<sup>45</sup> J. van Goor, “Seapower, Trade, and State-Formation: Pontianak and the Dutch,” in J. van Goor, ed., *Trading Companies in Asia, 1600–1830* (Utrecht, 1986), 83–106.

<sup>46</sup> The masses of printed and manuscript material on this campaign await full analysis; for a brief account, see John E. Wills, Jr., “China’s Farther Shores: Continuities and Changes in the Destination Ports of China’s Maritime Trade, 1680–1690,” in Ptak and Rothermund, *Emporia*, 53–77, at 57–60.

<sup>47</sup> Mary Anne Weaver, “In the Sultan’s Palace,” *New Yorker* (October 7, 1991): 56–93, at 69.

<sup>48</sup> For exceptionally fine non-Eurocentric use of Dutch and Javanese sources, see M. C. Ricklefs, *Jogjakarta under Sultan Mangkubumi, 1749–1792: A History of the Division of Java* (London, 1974).

expansion of its consumption in Europe, well beyond the limited capacity of the original production areas in Yemen.<sup>49</sup> These early examples of the great nineteenth and twentieth-century colonial monoculture and industrial-world consumption of Third World agricultural products deserve far more study than they have received.

For the maritime history of the rest of Southeast Asia, current knowledge is much less satisfactory. One excellent study has been done of the vigorous trade of the Portuguese, Japanese, and Chinese on the southern coast of Vietnam.<sup>50</sup> Siam, relatively open to foreigners, its monarchs involved in maritime trade, its Chinese settlers very influential and prime managers of its important "tributary" trade with China, deserves more study. The story of the rise and fall of French influence and of the power of the Greek adventurer Constantine Phaulkon in Siam during the 1680s is perhaps the most astonishing of the many improbable melodramas of European power and adventure in maritime Asia.<sup>51</sup>

The Philippines have a highly distinctive and important place in the history of maritime Asia, as a conduit for American silver into the Asian economies, the locus of an important European-Chinese-indigene interaction, and the one part of Asia that became Roman Catholic. This fascinating story has been largely ignored in "European expansion" surveys. The later history of the islands was so profoundly shaped by the projection across the Pacific of many of the policies and institutions of Spanish America, including the cumbersome structure of law and bureaucracy and the great importance of missionary enterprise, that the indigenization of this history has been unusually difficult.<sup>52</sup> Manila was as much a "Chinese colonial town" as Batavia.<sup>53</sup> American silver came thence from Acapulco primarily to pay for Chinese goods, and its greatest effects were on the economy of China.<sup>54</sup> For the history of the indigenous people of the islands after the Spanish intrusion, much remains to be done.<sup>55</sup> Some of the potentialities of the study of the massive

<sup>49</sup> W. Rummelink, "Expansion without Design: The Snare of Javanese Politics," *Itinerario*, no. 1 (1988): 111–28; G. J. Knapp, "Coffee for Cash: The Dutch East India Company and the Expansion of Coffee Cultivation in Java, Ambon and Ceylon, 1700–1730," in van Goor, ed., *Trading Companies in Asia*, 33–49.

<sup>50</sup> Pierre-Yves Manguin, *Les Portugais sur les côtes du Viêt-nam et du Campá* (Paris, 1972).

<sup>51</sup> Jurrien van Goor, "Merchant in Royal Service: Constant Phaulkon as Phraklang at Ayuthaya, 1683–1688," in Ptak and Rothermund, *Emporia*, 445–65; George Vinal Smith, *The Dutch in Seventeenth-Century Thailand* (Detroit, Mich., 1977); Claude de Bèze, S.J., E. W. Hutchinson, ed. and trans., *1688: Revolution in Siam* (Hong Kong, 1968); Sarasin Viraphol, *Tribute and Profit: Sino-Siamese Trade, 1652–1853* (Cambridge, Mass., 1977); Jennifer Cushman, "Fields from the Sea: Chinese Junk Trade with Siam during the Late Eighteenth and Early Nineteenth Century" (Ph.D. dissertation, Cornell University, 1975).

<sup>52</sup> Charles H. Cunningham, *The Audiencia in the Spanish Colonies, as Illustrated by the Audiencia of Manila (1583–1800)* (Berkeley, Calif., 1919); John L. Phelan, *The Hispanization of the Philippines* (Madison, Wis., 1967); H. de la Costa, S.J., *The Jesuits in the Philippines, 1581–1768* (Cambridge, Mass., 1961).

<sup>53</sup> Robert R. Reed, *Colonial Manila: The Context of Hispanic Urbanism and Process of Morphogenesis* (Berkeley, Calif., 1978); Alfonso Felix, Jr., *The Chinese in the Philippines*, 2 vols. (Manila, 1966). For a comparison of changes in a number of overseas Chinese centers in one period of change, see Wills, "China's Farther Shores."

<sup>54</sup> W. L. Schurz, *The Manila Galleon* (New York, 1939); Pierre Chaunu, *Les Philippines et le Pacifique des Ibériques (XVI<sup>e</sup>, XVII<sup>e</sup>, XVIII<sup>e</sup> siècles)* (Paris, 1960); William S. Atwell, "International Bullion Flows and the Chinese Economy circa 1530–1650," *Past and Present*, 95 (May 1982): 68–90.

<sup>55</sup> See, for example, William H. Scott, *The Discovery of the Igorots: Spanish Contacts with the Pagans of Northern Luzon* (Quezon City, 1974).

cultural changes brought about by the missionaries were opened up in John Leddy Phelan's fine book *The Hispanization of the Philippines*, which shows how Filipino responses to Christian evangelism were shaped by their pre-conquest heritages in ways as various as the importance of cults of holy water and the social importance of godparenthood. Vicente Rafael's *Contracting Colonialism* reveals how the missionaries' efforts to find clear and univocal translations of Christian terms and to establish orderly administration of confession and the sacraments was an attempt at a hierarchical, authoritarian restructuring of Tagalog concepts of relations among people, between people and spirits, and between life and death. The converts in turn found ways to subvert these processes, exasperating their confessors with their indirection and evasion. Some readers will become suspicious when Jacques Derrida and Michel Foucault are quoted and European rationalism is found uniquely bent on systematic power over others, but in this fascinating book the evidence of the links between rationalism and will to dominance in Spanish attitudes is plentiful and convincing. Along with Susan Bayly's ethnohistory and N. Standaert's indigenized study of elite cultural relations, Rafael's work lets us see as never before the contributions of mission history to non-parochial cultural history.

The paradoxes and contradictions of efforts to break from the "European expansion" paradigm do not abate in studies of the maritime histories of China, Japan, and Korea. I have already noted the fundamental contributions of Chinese emigrants to maritime enterprise and political development in Siam and at Batavia and Manila. These Chinese seafarers were also deeply involved in the spectacular rise and fall of a Dutch colony on Taiwan from 1624 to 1662, on which abundant Dutch materials now are being published. The pattern of triangular relations—among local people with little capacity to organize resistance, Chinese settlers, merchants, and tax-farmers, and a small and passive European community focused on an entrepôt trade with the Chinese—was strikingly similar to that at Manila. The Dutch even had one of their few substantial missionary efforts on Taiwan.<sup>56</sup>

THE "MARITIME CHINA" of coastal and overseas trade was marginal to the politics of the empire and sometimes organized in ways quite different from the land-centered bureaucratic system.<sup>57</sup> At several key junctures from the early 1400s on, the rulers of the empire sensed in that maritime world sources of

<sup>56</sup> The best summary in English is John R. Shepherd, *Statecraft and Political Economy on the Taiwan Frontier, 1600–1800* (Stanford, Calif., 1993). Dutch sources may be sampled in translation in William Campbell, *Formosa under the Dutch* (London, 1903). The major source publication now in progress is J. L. Blussé, et al., eds., *De Dagregisters van het Kasteel Zeelandia, Taiwan, 1629–1662, Deel I: 1629–1641* (The Hague, 1986). See also Thomas O. Höllman, "Formosa and the Trade in Venison and Deer Skins" in Ptak and Rothermund, *Emporia*, 263–90.

<sup>57</sup> John E. Wills, Jr., "Maritime China from Wang Chih to Shih Lang," in Jonathan D. Spence and John E. Wills, Jr., eds., *From Ming to Ch'ing: Conquest, Region, and Continuity in Seventeenth-Century China* (New Haven, Conn., 1979), 204–38; Ng Chin-Keong, *Trade and Society: The Amoy Network on the China Coast, 1683–1735* (Singapore, 1983); Pin-tsun Chang, "The First Chinese Diaspora in Southeast Asia in the Fifteenth Century"; and Ng Chin-Keong, "The Case of Ch'en I-lao: Maritime Trade and Overseas Chinese in Ch'ing Policies," in Ptak and Rothermund, *Emporia*, 13–29, 373–400.



challenge and disorder that could be most effectively dealt with by quarantine. Restriction of European and other foreign presences on the Chinese coast is best seen as part of a defensiveness that was deeply rooted in this wariness toward indigenous seafarers and, indeed, toward all Chinese who might ally with foreign intruders; not trusting their own people to limit and neutralize foreign contact, as Indians so often did, Chinese rulers sought to restrict foreign presence to border areas and to keep it under strict control even there. Only a few episodes in Chinese handling of European contacts were affected by the hierarchical institutions and preconceptions of the "tribute system"; the Chinese did have their own concepts of world order, but Western scholars' emphasis on them has been not so much an indigenization as a projection of Western concepts of an order of equal sovereign states and concern with any violation of that order.<sup>58</sup>

Chinese statesmen understood the potential fiscal and economic importance of foreign trade and permitted it when it could be managed in ways compatible with their wariness of the maritime Chinese and of uncontrolled Chinese connections with foreigners. Thus the Portuguese were allowed to settle on the tiny, easily controlled peninsula of Macao and to carry on from about 1570 to 1638 a rich trade with Japan, which the Ming authorities found far preferable to encouraging maritime Chinese involvement with the dangerous Japanese.<sup>59</sup> The huge eighteenth-century export of tea from Canton, one of the backbones of European trade in maritime Asia, was carefully controlled by the Ch'ing authorities to minimize the disturbances that might arise from bad debts or poor quality control. Chinese merchants, products of a sophisticated and rapidly expanding commercial economy, provided large quantities of goods of reliable quality; in contrast to India, the Europeans neither were allowed nor expressed any great desire for close control over the processes of production.<sup>60</sup>

Defensiveness toward the threat of cultural contamination and political subversion by foreign missionaries was a constant feature of Chinese policy. We have several recent studies that provide valuable accounts of the broad contexts, cultural, social, and political, of missionary efforts and Chinese reactions to

<sup>58</sup> John E. Wills, Jr., *Pepper, Guns, and Parleys: The Dutch East India Company and China, 1662–1681* (Cambridge, Mass., 1974), chap. 6; Wills, *Embassies and Illusions: Dutch and Portuguese Envoys to K'ang-hsi, 1666–1687* (Cambridge, Mass., 1984); Wills, "Tribute, Defensiveness, and Dependency: Uses and Limits of Some Basic Ideas about Mid-Ch'ing Foreign Relations," *Annals of the Southeast Conference of the Association for Asian Studies*, 8 (1986): 84–90, rpt. in *American Neptune*, 48 (Fall 1988): 225–29.

<sup>59</sup> C. R. Boxer, *Fidalgos in the Far East, 1550–1770* (1948; rpt. edn., London, 1968); Boxer, *The Great Ship from Amacon: Annals of Macao and the Old Japan Trade, 1555–1640* (Lisbon, 1959); George B. Souza, *The Survival of Empire: Portuguese Trade and Society in China and the South China Sea, 1630–1754* (Cambridge, 1986). For summaries of Portuguese and other maritime European relations with Ming China, see John E. Wills, Jr., "Relations with Maritime Europeans, 1514–1662," in *Cambridge History of China*, vol. 8, in press, also to appear in Wills, ed., *China and Maritime Europe, 1500–1800: Trade, Settlement, Diplomacy, and Missions* (Cambridge, in press).

<sup>60</sup> H. B. Morse, *Chronicles of the East India Company Trading to China*, 5 vols. (Oxford, 1926–29); Louis Dermigny, *La Chine et l'Occident: Le commerce à Canton au XVIII<sup>e</sup> siècle*, 3 vols. and album (Paris, 1964); C. J. A. Jörg, *Porcelain and the Dutch China Trade* (The Hague, 1982); Hoh-cheung Mui and Lorna H. Mui, *The Management of Monopoly: A Study of the East India Company's Conduct of Its Tea Trade, 1784–1833* (Vancouver, 1984); J. L. Cranmer-Byng and John E. Wills, Jr., "Ch'ing Trade and Diplomacy with Maritime Europe, 1644–c. 1800," *Cambridge History of China*, vol. 9, in press, also to appear in Wills, ed., *China and Maritime Europe*; Kuo-tung Anthony Chen, "The Insolvency of the Chinese Hong Merchants, 1760–1843" (Ph.D. dissertation, Yale University, 1990).

them.<sup>61</sup> Standaert's book is unique among them in the care with which it describes certain intellectual elite circles at a time of new trends and a general sense of moral and political crisis, and shows how one intellectual, dissatisfied with his previous Buddhist quests and deeply impressed by the personal commitment and moral rigor of the missionaries, made the amazing leap of conversion to Christianity. There were several other distinguished intellectual converts in this generation but few in later generations. Looking at this decline in the light of recent advances in the understanding of their Chinese contexts, it is not hard to see how the emergence of a coherent naturalistic monism in philosophy and the revival of traditional moralistic fervor in the struggles against eunuch tyranny and Ch'ing conquest reduced the sense of moral and intellectual crisis that had moved Yang Tingyun and others to the desperate act of conversion.<sup>62</sup>

The China-India contrast in ways of dealing with foreigners is only one of a number of exceptional challenges to comparative analysis of the relations of China, Europe, and maritime Asia. Another is the contrast in defensive responses between China and Japan. Japan in the late sixteenth century exhibited impressive positive responses to Christianity, openness to European trade, and energetic development of maritime trade in Japanese ships. But by 1640, it had turned to "closed country" policies, ruthlessly forbidding and extirpating Christianity, expelling the Portuguese because of their connection with the missionaries, and confining Dutch trade to the one port of Nagasaki. The challenges of fitting an understanding of these changes into our rapidly changing interpretations of early modern Japan are formidable. Japan after 1550 was evolving a new type of political order that can be fruitfully compared to the interventionist, revenue-increasing military states discussed by Pearson, C. A. Bayly, and others. Other changes included dramatic economic growth and urbanization, the emergence of an urban popular culture, and growing concern with the relations among Buddhism, Confucianism, and Japanese tradition and identity. Historians now have a superb guide to this complex set of changes in the volume on early modern Japan of the *Cambridge History of Japan*. The volume contains two excellent chapters on foreign relations by Jurgis Elisonas, the author of an earlier path-breaking study of Japanese reactions to Christianity.<sup>63</sup> Steeped in the primary sources, with fresh information and insight on every page, these chapters

<sup>61</sup> John D. Young, *Confucianism and Christianity: The First Encounter* (Hong Kong, 1983); Jonathan D. Spence, *The Memory Palace of Matteo Ricci* (New York, 1984); Jacques Gernet, *China and the Christian Impact* (Cambridge, 1985); and Charles E. Ronan, S.J., and Bonnie B. C. Oh, eds., *East Meets West: The Jesuits in China, 1582–1773* (Chicago, 1988). A primary text of first importance now is available in English: Matteo Ricci, S.J., *The True Meaning of the Lord of Heaven* (T'ien-chu shih-i), Edward J. Malatesta S.J., ed., Douglas Lancashire and Peter Hu Kuo-chen, S.J., trans. (St. Louis, Mo., 1985). An excellent summary of post-1644 missions in China is John W. Witek, S.J., "Catholic Missions and the Chinese Reactions to Christianity," to appear in *Cambridge History of China*, vol. 9, and also in Wills, ed., *China and Maritime Europe*.

<sup>62</sup> I have developed this argument in "Brief Intersection: Changing Contexts and Prospects of the Christian-Chinese Encounter from Ricci to Verbiest," to be published in a volume of studies of Ferdinand Verbiest, S.J., by the Ferdinand Verbiest Foundation, Leuven.

<sup>63</sup> Jurgis Elisonas, "The Inseparable Trinity: Japan's Relations with China and Korea"; and "Christianity and the Daimyo," in John W. Hall and James L. McClain, eds., *Cambridge History of Japan*, Volume 4 (Cambridge, 1991), 235–372; George Elison, *Deus Destroyed: The Image of Christianity in Early Modern Japan* (Cambridge, Mass., 1973). See also C. R. Boxer, *The Christian Century in Japan, 1549–1650* (Berkeley, Calif., 1951).

and this volume make Japan one of the most accessible cases of the interpretative shift from European expansion to interactive emergence. The positive commercial and religious responses of the late sixteenth century can be seen as facets of an age of political division in which a number of regional leaders sought to increase their wealth by attracting the Portuguese ships to their harbors, and conversions to Christianity were one of a number of manifestations of subversive popular religious organization and challenge to authority; the turn to control of trade and suppression of Christianity then can be viewed as facets of the general creation of strongly centralized polities and crushing of religious subversion. It is unfortunate, however, that this volume gives no connected coverage to the changes in the size and composition of Japan's foreign trade, and the energetic political efforts to control them, from the 1640s on,<sup>64</sup> or to the energetic efforts of some eighteenth-century intellectuals to use the small window on the world offered by Dutch trade to learn about European medicine, science, and even art.<sup>65</sup>

In light of the achievements of the *Cambridge History* volume and the other important topics that have been opened up for Western readers, Massarella's book seems an odd throwback to a Eurocentric approach. Concentrating on the English efforts to open trade with Japan, it includes a great deal of detailed and unfocused summary of English documents. Only a few sections, notably a chapter on English-Japanese social relations in the port of Hirado, offer sustained analysis and attention to Asian contexts.

Another important indigenizing contribution has been Ronald Toby's path-breaking study of Japan's diplomatic stance toward its Asian neighbors. Toby argues that Japan remained diplomatically active, involved in foreign trade, in touch with and aware of the outside world, after 1640.<sup>66</sup> I would add that the highly focused energy and political control with which the Japanese leaders first promoted Japanese foreign trade and then forbade it, continually adjusted control measures at Nagasaki, and sought to extirpate Christianity are expressions of a focused political will and a sensitivity to Japan's changing position in the world that had no counterpart in China. China's overseas trade never was shut down as effectively as Japan's was after 1640, nor were the proscriptions of Christianity in China enforced with such dreadful thoroughness.

THE PICTURE SKETCHED in this essay of the interactive emergence of European-dominated interrelations in maritime Asia is based on the indigenized historiographies of various parts of Asia, all of which emphasize the internal dynamisms and processes of change of the various societies. I have noted many episodes of

<sup>64</sup> The most thorough study now available in English is Robert L. Innes, "The Door Ajar: Japan's Foreign Trade in the Seventeenth Century" (Ph.D. dissertation, University of Michigan, 1980). For an interesting quantifying interpretation based on erratic knowledge of the literature on Japan, see Dennis O. Flynn, "Comparing the Tokagawa [sic] Shogunate and Hapsburg Spain: Two Silver-Based Empires in a Global Setting," in Tracy, *Political Economy*, 332–59.

<sup>65</sup> For a fine introduction, see Donald Keene, *Japan's Discovery of Europe, 1720–1830* (Stanford, Calif., 1969).

<sup>66</sup> Ronald P. Toby, *State and Diplomacy in Early Modern Japan: Asia in the Development of the Tokugawa Bakufu* (Princeton, N.J., 1984).

impressive organization of maritime power and commerce, from Oman to Japan. The failures of most of them to sustain themselves and challenge the long drift to European domination of Asian waters were results of the contingencies of their own political and economic histories. The Europeans had a technological edge at certain times and in certain ways, but it was not insuperable. Far more important were the organizational cohesion and staying power of their state and corporate organizations, their effective interactions with Asians, their exploitation of Asians' talents and practices. Mobilizing state structures did emerge in India but long after the Europeans had intruded on the lands around the Indian Ocean. The sophistication and staying power of the Chinese imperial state made regional state-building tendencies neither feasible nor essential for defense. State building in Japan led to withdrawal into the "closed country."

This non-Eurocentric historiography offers much of interest and importance for historians of Europe, who can see in it, as in C. A. Bayly's *Imperial Meridian*, their familiar subject made strange by new contexts and comparisons. Asianists, in turn, find here a set of topics and issues about which they can learn from Europeanists and, perhaps even more surprisingly, from each other, as students of Japan, India, the Philippines, and the rest sort out the comparisons and interactions sketched here. I have suggested the phrase "interactive emergence" to describe the historical phenomena here discussed; more and more, it seems appropriate for their historiography as well.

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*Review Article*

The Frontier Trail: Rethinking Turner and  
Reimagining the American West

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JOHN MACK FARAGHER

William Cronon, George Miles, and Jay Gitlin, eds., **Under an Open Sky: Rethinking America's Western Past** (New York: W. W. Norton, 1992).

Richard W. Etulain, ed., **Writing Western History: Essays on Major Western Historians** (Albuquerque: University of New Mexico Press, 1991).

Patricia Nelson Limerick, Clyde A. Milner II, and Charles E. Rankin, eds., **Trails toward a New Western History** (Lawrence: University Press of Kansas, 1991).

Gerald D. Nash, **Creating the West: Historical Interpretations 1890–1990** (Albuquerque: University of New Mexico Press, 1991).

Donald Worster, **Under Western Skies: Nature and History in the American West** (New York: Oxford University Press, 1992).

THE “NEW WESTERN HISTORY” has been getting a lot of press lately. A recent cover story in *U.S. News and World Report* announced that historians had now shown that the American West was not “some rough-hewn egalitarian democracy, where every man had a piece of land and the promise of prosperity, but a world quickly dominated by big money and big government”; not a land “where the sodbuster might dwell in sweet harmony with nature, but a nearly unmitigated environmental catastrophe”; not a society of close-knit pioneer families, but one in which men, women, and children were “torn apart by the great desert emptiness of the West.” These revisions led to an inevitable conclusion: “The Turnerian view of the West is falling apart these days.”<sup>1</sup>

This is news? In a world of dizzying intellectual fashion changes—from modernism to postmodernism to claims of the “end of history” itself—it may come as something of a surprise that historians of the American West have taken so long to overturn the interpretation of a century-old conference paper.

Delivered in 1893 to a meeting of the American Historical Association, “The Significance of the Frontier in American History” by Frederick Jackson Turner

I wish to thank the graduate students who considered the “new western history” with me in a seminar at the University of Massachusetts, Amherst, in the spring of 1992.

<sup>1</sup> Miriam Horn, “How the West Was Really Won,” *U.S. News and World Report*, May 21, 1990.



(then a thirty-two-year-old assistant professor at the University of Wisconsin) was the classic expression of the Turnerian view: that "the existence of an area of free land, its continuous recession, and the advance of American settlement westward, explain American development," that this frontier accounted for American democracy and character, and that the frontier had now closed, with uncertain consequences for the American future. By 1910, the year Turner assumed the presidency of the AHA as well as a chair at Harvard, the frontier thesis had become the commanding view of the American past, a position it held for more than half a century. It became the most familiar model of the American past, the one taught in school, extolled by politicians, and screened at the local movie theater each Saturday afternoon. Turner's collected essays, *The Frontier in American History*, won the Pulitzer Prize in 1920; and in 1952 the book placed second on a list of historians' favorites, sandwiched between Vernon Parrington's *Main Currents in American History* and Charles Beard and Mary Beard's *Rise of American Civilization*. As late as 1964, a survey of nearly three hundred American historians found Turner's ideas "still dominant."<sup>2</sup>

Reports of Turner's fall from grace nevertheless struck me as a headline from the past. At the time of that 1964 survey, I was a California sophomore studying history, and after graduation I spent several years bumping from one history graduate program to another until finally settling at the institution where I completed my dissertation. In every instance, my professors in the history of the American West always read Turner as a primary source, a fascinating intellectual road map to the American *fin de siècle*, but certainly not a model for their own work in western history. I came to respect the professional work of the late Ray Allen Billington, a Turnerian who encouraged much good western history through his leadership of the Western History Association (established in 1961) and his *Histories of the American Frontier* series; but I admit that I found his defense of the frontier thesis something of an intellectual curiosity. We graduate students had other things on our minds, inspired by the prospect of applying the methods of the new social history to the West and by the challenge of re-envisioning the relationship of region to nation and to the global history of colonization.

Thus I was surprised that a number of today's most prominent western historians believe the field remains "stuck in a Turnerian rut." In the words of Donald Worster, author of the provocative *Rivers of Empire: Water, Aridity, and the Growth of the American West* (1985), "Turner presides over western history like a Holy Ghost." At meetings of the Western History Association, Worster remarks, "heads still bowed dutifully at the name of Frederick Jackson Turner, and a few still crossed themselves in reverence." Sentiments like these seem to have resuscitated the few remaining Turnerians, who have risen up angry. Martin Ridge, who after Billington's death revised his textbook, *Westward Expansion: A History of the American Frontier* (original, 1949; 5th edition, 1982), dares historians like Worster to "explain what is new about their work other than their personal

<sup>2</sup> Frederick Jackson Turner, *The Frontier in American History* (New York, 1920), 1; William N. Davis quoted in Gerald D. Nash, *Creating the West: Historical Interpretations 1890–1990* (Albuquerque, N.M., 1991), 74.

assumptions and value judgments." Arguing that they have little to offer as a replacement for Turner's interpretation, Ridge fires back that they "may find that it is exceedingly difficult to bury his ghost."<sup>3</sup>

The arguments over western history sometimes get ill-tempered. Turnerians unfairly represent the new western history as all doom and gloom, while the anti-Turnerians exaggerate the strength of their opponents, although it turns out that Turner's spirit, if not his ghost, is still around. But the current debate, rehearsed in this bumper crop of new books, is testimony to an extraordinary burst of intellectual energy. Over the last thirty years, historians have reimagined the history of the American West.

FOR AN OVERVIEW OF THE DEBATE that has continued in the century since Turner delivered his paper, turn to *Creating the West* by Gerald D. Nash, professor of history at the University of New Mexico and author of one of the first histories of the modern West, *The American West in the Twentieth Century: A Short History of an Urban Oasis* (1973). Nash organizes his argument around the proposition that the currents of historical interpretation are a function of generational differences; when historians, at early stages of their careers, share common perceptions of decisive events, they form a generational consensus of opinion. "The visions of the West which successive generations had in mind," he writes, "were as determining in shaping their particular interpretations as the historical record."<sup>4</sup> The generation for which Turner became spokesman, for example, was shaped by its perception of the American crisis of the 1890s—the farmer and worker upsurge, the economic depression, the end of continental expansion and the beginning of overseas imperialism—jolting episodes in the transition from rural and agricultural to urban and industrial society. Hailing mostly from country and small-town backgrounds, this generation of historians, writes Nash, found appealing an explanation like Turner's, which located America's historic meaning in its rural past. The influence of Turnerian views in the early century, he concludes, had less to do with the intellectual power of the synthesis or the compelling nature of the evidence than with a shared sense of generational nostalgia.

Here, at the beginning of his book, Nash makes the best case for his thesis—perhaps because by paying relatively little attention to the scholarly and intellectual trends out of which Turner and his contemporaries emerged in the late nineteenth century, he simplifies the problem of distinguishing what was unique about their generation. But with his subsequent cohorts—three generations of historians demarcated by the shaping experience of the first and second world wars and "the sixties"—he must confront messy transitions, and his argument is less persuasive.

Before detailing that critique, however, I wish to acknowledge how useful

<sup>3</sup> Donald Worster quoted in T. R. Reid, "Shootout in Academia over History of U.S. West," *Washington Post*, October 10, 1989; Donald Worster, *Under Western Skies: Nature and History in the American West* (New York, 1992), 8; Martin Ridge, "Frederick Jackson Turner and His Ghost: The Writing of Western History," *Proceedings of the American Antiquarian Society*, n.s. 101 (1991): 76.

<sup>4</sup> Nash, *Creating the West*, 259.

Nash's book is as a guide to the debate and how fully it documents the early and sustained challenge to Turnerian views. Critics began attacking them in earnest in the 1920s, when an important component of intellectual radicalism took the form of a kind of inverted frontier thesis, invoking the western past to account for many of the negative aspects of American civilization. The frontier, John Dewey declared in 1922, was most notable for its "depressing effect upon the free life of inquiry and criticism." Other scholars began to question what one of them called "the shibboleth of the frontier" in American historiography, picking away at Turner's fuzzy definitions, his internal inconsistency, his sometimes faulty logic. Charles Beard, with a thesis of his own to promote, blasted "orthodox historians" for dwelling on the frontier thesis to the exclusion of nearly all other interpretations; "the tabu is almost perfect," he remarked, "the American Historical Association officially is as regular as Louis XVI's court scribes." In the 1930s, following Turner's death in 1932, critics pummeled his work unmercifully. Numerous studies appeared with alternative readings of his historical evidence: there had never really been any "free land" (Paul Wallace Gates), the idea of a vanished frontier was erroneous (Isaiah Bowman), democratic institutions had been imported from the East (Louis B. Wright), the West had never acted as a safety valve for urban discontent (Fred A. Shannon), settlement had fostered community rather than individualist values (Mody C. Boatright).<sup>5</sup>

Of even greater significance was the emergence of a number of alternative frameworks for western history. Herbert Eugene Bolton called for a multinational and multiethnic history of the North American West, and Joseph Kinsey Howard wrote an epic narrative of the métis people of the northern plains that called attention to some of the continent's other expanding peoples. Walter Prescott Webb made the case for the West as an arid region and joined with Bernard DeVoto to charge that it was a "plundered province." James C. Malin produced pioneering work in western social and environmental history, while Carey McWilliams sought to inaugurate the history of the urban West. Earl Pomeroy insisted that the Westerner was "fundamentally imitator rather than innovator," and Henry Nash Smith investigated the cultural sources of the American myth of the West, including the deep background of the frontier thesis itself. With the exception of DeVoto, Howard, and McWilliams, there are insightful intellectual portraits of these and other men who shaped western historiography in Richard Etulain's excellent collection, *Writing Western History*.<sup>6</sup>

<sup>5</sup> Dewey and Beard quoted in Nash, *Creating the West*, 22, 24; John C. Almack, "The Shibboleth of the Frontier," *Historical Outlook*, 16 (May 1925): 197–202; Paul Wallace Gates, "The Homestead Law in an Incongruous Land System," *AHR*, 41 (July 1936): 652–81; Isaiah Bowman, *The Pioneer Fringe* (New York, 1931); Louis B. Wright, "American Democracy and the Frontier," *Yale Review*, 22 (1930): 349–65; Fred A. Shannon, "The Homestead Act and Labor Surplus," *AHR*, 41 (July 1936): 637–51; Mody C. Boatright, "The Myth of Frontier Individualism," *Southwestern Social Science Quarterly*, 22 (1941): 14–32.

<sup>6</sup> Herbert Eugene Bolton, "The Epic of Greater America," *AHR*, 38 (April 1933): 448–74; Joseph Kinsey Howard, *Strange Empire: Louis Riel and the Metis People* (New York, 1952); Walter Prescott Webb, "The American West: Perpetual Mirage," *Harper's*, 214 (May 1957): 25–31; Bernard DeVoto, "The West: A Plundered Province," *Harper's*, 169 (August 1934): 355–64; James C. Malin, "The Turnover of Farm Population in Kansas," *Kansas Historical Quarterly*, 4 (1935): 339–72, and "The Adaptation of the Agricultural System to Sub-Humid Environment," *Agricultural History*, 10 (1936): 118–41; Carey McWilliams, "Introduction," in *Rocky Mountain Cities*, Ray B. West, ed. (New York,

Thus a debate over interpretation of the western past took form. There can be little doubt about the shaping influence of the times in which these historians lived—the power of an iconoclastic spirit during the 1920s, the concern with economic depression during the 1930s. Turner himself recognized the desirability of historical presentism, announcing in one of his first published essays that “each age writes the history of the past anew with reference to the conditions uppermost in its own time.” The frontier thesis of the 1890s retained a strong following into mid-century, but it did not go unchallenged. “Such is the force,” William N. Davis concluded after considering the results of his poll of historians in 1964, “of the anti-, un-, and non-Turnerian groups, that it would appear only a matter of time until they attain majority status.”<sup>7</sup>

It is much less clear, however, that these shifts in interpretation followed generational boundaries. Nash’s model fails to explain numerous cases. For example, when Arthur Schlesinger, Sr., began a distinguished career as one of the most prominent historians of the interwar years, he stated his convictions as merely a modification of Turnerian views, writing that “the two grand themes of American history are, properly, the influence of immigration upon American life and institutions, and the influence of the American environment, especially the frontier in the early days.” Some twenty years later, however, in his presidential address to the AHA in 1942, Schlesinger rejected the frontier thesis out of hand, arguing that the American “is the product of the interplay of his Old World heritage and New World conditions.” His views, it seems, were shaped less by his generation than by the debate of the intervening decades. Moreover, Schlesinger’s student, Ray Allen Billington, began his career as one of those “anti-, un-, or non-Turnerians,” remarking in 1942 that the “most glaring of the Turner errors is probably his stress on the lone frontiersman.” Billington, too, changed his mind.<sup>8</sup>

Nash does not even grant that such cases are anomalous, blithely ignoring all of them as he moves toward a sweeping statement of his theme. “Essentially,” he concludes, successive generations of historians “were engaged in replacing one set of myths with another—with those that seemed more satisfying to their own generation,” a view that amounts to a *ne plus ultra* of historical agnosticism and

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1949); Earl Pomeroy, “Towards a Reorientation of Western History: Continuity and Environment,” *Mississippi Valley Historical Review*, 41 (1955): 579–600; Henry Nash Smith, *Virgin Land: The American West as Symbol and Myth* (Cambridge, Mass., 1950). See Donald E. Worcester, “Herbert Eugene Bolton: The Making of a Western Historian”; Elliott West, “Walter Prescott Webb and the Search for the West”; Allan G. Bogue, “James C. Malin: A Voice from the Grassland”; Michael P. Malone, “Earl Pomeroy and the Reorientation of Western American History”; Lee Clark Mitchell, “Henry Nash Smith’s Myth of the West”; all in *Writing Western History: Essays on Major Western Historians*, Richard W. Etulain, ed. (Albuquerque, N.M., 1991). The editors of *Trails toward a New Western History* (Lawrence, Kan., 1991), Patricia Nelson Limerick, Clyde A. Milner II, and Charles E. Rankin, dedicate their collection to Carey McWilliams, but unfortunately there is no substantive discussion of his work in the text.

<sup>7</sup> Turner quoted in William Cronon, “Turner’s First Stand: The Significance of Significance in American History,” in Etulain, *Writing Western History*, 75; Davis quoted in Nash, *Creating the West*, 74.

<sup>8</sup> Schlesinger and Billington quoted in Nash, *Creating the West*, 25, 41, 46.

relativism. Does Nash mean to suggest that we should consider his own historical work on the West as simply another "myth"?<sup>9</sup>

That he does not, that he pursues an agenda of his own, becomes evident as he employs the generational thesis like a club to beat the latest generation of western historians. Seemingly oblivious to the extreme relativism of his own approach, Nash criticizes today's young historians as "relativists with a vengeance," accusing them of "openly abandon[ing] any effort to retain a measure of historical objectivity." "New Left" historians and "those who followed in their path without an explicit ideological commitment [fellow travelers?] found little in the West's past that was right, but saw a great deal of what they considered wrong." But not once does Nash take up a detailed criticism of the work he so deplores, not once does he show why he thinks it is wrong by reference to contrary evidence or faulty logic. His critique is confined to outrage and *ad hominem* attack. "Nurtured by unprecedented financial support in their educational development and research grants not available to previous generations of academics," the sixties generation burst onto the scene and "rudely disrupted" the proceedings. Nash condemns their "disservice to the profession" and even implies that they are betraying their country. "If historians are also keepers of a nation's soul, the custodians of its sense of identity, one-sided indictments can serve the function of destroying the very fabric of national identity." Mean-spirited rhetoric of this sort debases and trivializes the debate.<sup>10</sup>

To be fair, Nash is not alone in insisting that controversies over the western past be cast as a generational struggle. According to Donald Worster, western history has been remade during the last two decades by the "younger generation, shaken by the experience of Vietnam and other national disgraces," although he qualifies this by adding that he does not mean to say that this is solely the achievement of scholars under the age of fifty. Indeed. The publications first articulating the themes now identified with the "new western history" were in fact written by historians of previous generations. Before I, and most of my contemporaries, had left graduate school, scholars such as A. Irving Hallowell, Jack D. Forbes, David M. Potter, Roger Daniels, Richard Maxwell Brown, Wilbur Jacobs, and Robert V. Hine had challenged Turnerian assumptions and begun to detail the perspectives of the many diverse communities of the West—not only Anglo-Americans but Indian, Hispanic, and métis peoples, Asian and European immigrants, women as well as men. Whether they realize it or not, most of today's western historians build on the contributions of their anti-, un-, or non-Turnerian predecessors. By failing to give full recognition to these pathbreaking studies, the generational thesis violates one of the cardinal rules of history: close attention to antecedents.<sup>11</sup>

<sup>9</sup> Nash, *Creating the West*, 259.

<sup>10</sup> Nash, *Creating the West*, 276, 79, 130, 262.

<sup>11</sup> Worster, *Under Western Skies*, 11–12. A. Irving Hallowell, "The Backwash of the Frontier: The Impact of the Indian on American Culture," in *The Frontier in Perspective*, Walker D. Wyman and Clifton B. Kroeber, eds. (Madison, Wis., 1957); Jack D. Forbes, "The Indian in the West: A Challenge for Historians," *Arizona and the West*, 1 (1959): 206–15, and "Frontiers in American History," *Journal of the West*, 1 (1962): 63–73; David M. Potter, "American Women and the American Character" [first published 1962], reprinted in *History and American Society: Essays of David M. Potter*, Don E. Fehrenbacher, ed. (New York, 1973); Roger Daniels, "Westerners from the East: Oriental Immigrants Reappraised," *Pacific Historical Review*, 35 (1966): 373–83; Richard Maxwell Brown, "Histor-



Writing in *Trails toward a New Western History*, Patricia Nelson Limerick, whose influential *Legacy of Conquest: The Unbroken Past of the American West* (1987) is a stimulating showcase of "new western history" arguments, places the participants in this debate into categories of "old" and "new," a variation on this generational framework. This sometimes requires considerable tailoring, for some old ideas, she admits, turn out to be "new." The frontier thesis was the cornerstone of the "old western history," but then Turner himself argued for rethinking the past in light of the present, which made him "new" as well; Walter Prescott Webb "was decidedly New in his emphasis on the West's limited water and decidedly Old in his patronizing treatment of Indians and Hispanics"; and so on. Are we likewise to dismiss some new ideas as "old"?<sup>12</sup>

The rhetoric of "new" and "old" seems simply a way of distinguishing between arguments one does and does not like. Alternative approaches to the history of the American West have existed among historians since the reconsideration of the frontier thesis began in earnest, and contrasting interpretations have less to do with "generations" than with social theory, moral values, and the angle and breadth of historical vision.

THE PROBLEM OF HISTORICAL VISION is prominent among a number of themes that run through the essays in these books. The Turnerians composed imperialist chronicles, success stories that, in good Turner fashion, told of "the advance of the pioneer into the wastes of the continent." Their collective narrative linked the rise of the American nation-state with cherished values like opportunity and freedom, with the certainty of white supremacy, and with the development of rugged masculinity.<sup>13</sup>

We now require what Sarah Deutsch, in *Under an Open Sky*, a festschrift honoring Yale University's Howard R. Lamar, calls "a new narrative form more appropriate to a pluralistic concept of history." In that same collection, George Miles observes that in rethinking the Indian past over the last several decades, ethnohistory has tended to depict Indians and whites in antithetical terms, making it difficult "to imagine an approach in which Indian history can be incorporated into the mainstream of American historiography." Because both groups contributed to the making of the West, he argues, historians interested in a common narrative need to become more attentive to interchange rather than conflict. Jay Gitlin suggests the necessity of analyzing the contrasts between the frontiers created by different European colonial powers in North America. "The

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ical Patterns of Violence in America," in *Violence in America: Historical and Comparative Perspectives*, Hugh Davis Graham and Ted Robert Gurr, eds. (New York, 1969); Wilbur Jacobs, "British Colonial Attitudes and Policies towards the Indian in the American Colonies," in *Attitudes of Colonial Powers toward the American Indian*, Howard Peckham and Charles Gibson, eds. (Salt Lake City, Ut., 1969), and "The Great Despoliation: Environmental Themes in American History," *Pacific Historical Review*, 47 (1978): 1-26; Robert V. Hine, *The American West: An Interpretive History* (Boston, 1973; 2d edn., 1984).

<sup>12</sup> Patricia Nelson Limerick, "The Trails to Santa Fe: The Unleashing of the Western Public Intellectual," in *Trails toward a New Western History*, 61.

<sup>13</sup> Turner quoted in Theodore Roosevelt, *The Winning of the West: Selections*, Harvey Wish, ed. (New York, 1962), xxi.

Spanish Empire evolved as an amalgam of people and places, and Native American communities had standing in that heterogeneous polity," while the French, with their isolated communities and their reliance on the fur trade, "had no choice but to recognize the sovereignty of Indian tribal communities and observe the boundaries of legal jurisdiction." Looking elsewhere besides the British-Indian frontier reveals a rich history of consort as well as conflict.<sup>14</sup>

Historians of western women are also building on prior scholarship. Peggy Pascoe, in *Trails toward a New Western History*, carefully acknowledges the contribution of the first wave of western women's history while noting its rather narrow preoccupation with "westering," a Turnerian view if there ever was one. "Now that it is impossible to believe that there is only one cultural theme in American history," she writes, "we're struggling to figure out how to write a history that does justice to them all." She argues for a focus on western women who were "intercultural brokers, mediators between two or more very different cultural groups," pointing to the work of Sylvia Van Kirk on women in fur trade society and of Sarah Deutsch on gender and ethnicity in the Southwest. I would add Pascoe's own examination of the relationships of Anglo-American women reformers to Omaha Indian women, polygamous Mormon wives, and Chinese prostitutes.<sup>15</sup>

Another theme in these essays commanding considerable attention is the history of the twentieth-century West, something the Turnerians ignored, with their focus on the closing of the continental frontier at the end of the last century. Here, too, today's western historians can look to a legacy of challenge to Turnerian views. During the 1890s, as the frontier thesis began to work its way into the historical imagination of this country, Adna F. Weber produced the first statistical study of the growth of American cities, showing that the West "was more heavily urban" than any other region except the Northeast. Noting the urban-rural dynamic at the heart of western settlement and development, he pointed out that "a Mississippi Valley empire rising suddenly into being without its Chicago and smaller centres of distribution is almost inconceivable." While this insight failed to inspire Turnerians, in Canadian history "metropolitanism" became the leading model for understanding the development of the western provinces. American western historians rediscovered the urban dimension in the postwar period, making it possible, as Carey McWilliams argued in 1949, to see "the West from a new point of view: from its centers, not its fringes; from the places where its interests are articulated, not where they are most dispersed." Earl Pomeroy,

<sup>14</sup> Sarah Deutsch, "Landscape of Enclaves: Race Relations in the West, 1865-1990"; George Miles, "To Hear an Old Voice: Rediscovering Native Americans in American History"; and Jay Gitlin, "On the Boundaries of Empire: Connecting the West to Its Imperial Past," all in *Under an Open Sky: Rethinking America's Western Past*, William Cronon, George Miles, and Jay Gitlin, eds. (New York, 1992), 112, 54, 75.

<sup>15</sup> Peggy Pascoe, "Western Women at the Cultural Crossroads," in *Trails toward a New Western History*, 56, 55. See Sylvia Van Kirk, *"Many Tender Ties": Women in Fur Trade Society in Western Canada* (1980; rpt. edn., Norman, Okla., 1983); Sarah Deutsch, *No Separate Refuge: Culture, Class, and Gender on an Anglo-Hispanic Frontier in the American Southwest* (New York, 1987); Peggy Pascoe, *Relations of Rescue: The Search for Female Moral Authority in the American West, 1874-1939* (New York, 1990).

Charles Gates, and others made further advances in western urban studies during the 1950s and 1960s.<sup>16</sup>

Focusing on the urban-rural connection breaks the distinction between "old West" and "new West," with a divide at the "closing" of the frontier in 1890. Patricia Limerick rightly insists on the continuity of western history across the centuries to the present; she repeats a call issued by western historians from James Malin to John Caughey, who twenty years ago urged his colleagues to end their "self imposed imprisonment in the early and antique West." But problems of periodization remain. As Michael P. Malone argues in *Trails toward a New Western History*, Turner may have misidentified the wrenching changes of the 1890s as a final closing of the frontier, but he was surely correct in identifying the period as a major transition in the relationship of the West to the nation, just as the expansionist 1840s were an earlier watershed. A consensus has developed among today's western historians that another such transition occurred during the period from 1933 to 1945, when massive federal infusions of capital transformed the region into an economic and cultural pacesetter for the rest of the nation.<sup>17</sup>

This argument, forcefully made by Gerald Nash in his influential books on the twentieth-century West, was also anticipated by earlier work. During the 1930s, Walter Prescott Webb, Bernard DeVoto, and Joseph Kinsey Howard argued that the West was, in DeVoto's words, "a plundered province," victimized by eastern capital, "looted, betrayed, sold out." But DeVoto was also among the first to recognize that the West had been transformed by World War II. "New Deal measures, war installations, and war industries have given the West a far greater and more widely distributed prosperity than it has ever had before," he wrote in 1946; "the ancient Western dream of an advanced industrial economy . . . is brighter than it has ever been before." But while the war certainly transformed southern California and other urban centers, the western interior remained a hinterland controlled by metropolitan elites. "Most western men and women may still be paying economic tribute today," writes Michael E. McGerr in *Under an Open Sky*, "but now they are paying more of it to other westerners."<sup>18</sup>

On this question of hierarchy and power in the modern West, Donald Worster

<sup>16</sup> Weber and McWilliams quoted in Nash, *Creating the West*, 160–61, 176. For "metropolitanism" in Canadian historiography, see W. L. Morton, "The Significance of Site in the Settlement of the American and Canadian Wests," *Agricultural History*, 24 (1951): 97–104; J. M. S. Careless, "Frontierism, Metropolitanism, and Canadian History," *Canadian Historical Review*, 35 (1954): 1–21; Careless, *Frontier and Metropolis: Regions, Cities, and Identities in Canada before 1914* (Toronto, 1989). See also Charles Gates, "The Role of Cities in the Westward Movement," *Mississippi Valley Historical Review*, 37 (1950–51): 277–78; Gates, "The Concept of the Metropolis in the American Western Movement," *Mississippi Valley Historical Review*, 49 (1962–63): 299–300; Earl Pomeroy, *The Pacific Slope: A History of California, Oregon, Washington, Idaho, Utah, and Nevada* (New York, 1966).

<sup>17</sup> Caughey quoted in Nash, *Creating the West*, 97; Limerick, "What on Earth Is the New Western History," and Michael P. Malone, "Beyond the Last Frontier: Toward a New Approach to Western American History," in *Trails toward a New Western History*, 86, 158. See Gerald Nash, *The American West Transformed: The Impact of the Second World War* (Bloomington, Ind., 1985); and Nash, *World War II and the West: Reshaping the Economy* (Lincoln, Neb., 1990).

<sup>18</sup> DeVoto quoted in Nash, *Creating the West*, 116, 118–19; Michael E. McGerr, "Is There a Twentieth-Century West?" in *Under an Open Sky*, 244. See Bernard DeVoto, "The West: A Plundered Province," *Harper's*, 169 (August 1934); and "The Anxious West," *Harper's*, 193 (December 1946); Walter Prescott Webb, *Divided We Stand: The Crisis of a Frontierless Democracy* (New York, 1937); Joseph Kinsey Howard, *Montana: High, Wide, and Handsome* (New Haven, Conn., 1943).

has the most to say. Indeed, in his new collection of essays, *Under Western Skies*, Worster has a great deal to say on a wide range of topics, making his one of the most powerful voices in western history today. The West has always been a supplier of raw materials—grain, meat, lumber, minerals, fuel—for markets somewhere else. But during the last century, he argues, two “ecological modes”—cattle raising and irrigation agriculture—have dominated the scene, both contributing to the growth of “concentrated power.” Ranching and agribusiness are large corporate enterprises, both dependent on the management of resources by federal agencies. This interlocking system fosters “hierarchy, concentrations of wealth and power, rule by expertise, dependency on government and bureaucracy.” Thus, more than any other region, the West has come under the sway of big capital and big government. For Worster, western history “best exemplifies the modern capitalistic state at work.”<sup>19</sup>

The environmental theme embedded in this analysis has become one of the most important of recent trends in western historiography, thanks to the likes of Worster, as well as other historians including Richard White and William Cronon. But it would be difficult to imagine their work without predecessors like Walter Prescott Webb, the pioneer of the environmental arguments about the West, or James Malin, whom Allan Bogue describes in his biographical essay as “the first historian to understand and make constructive use of ecological theory.” One of Malin’s projects was the study of human adaptation to the Great Plains. The Dust Bowl of the 1930s, he concluded, was the product of a “pioneering” culture. An opponent of the New Deal, he opposed government regulation or supervision and suggested that Plains farmers would do better once they more completely understood environmental limitations. While praising Malin as “the man who, more than any other, anticipated the emerging ecological synthesis in history,” Worster dissents from Malin’s conclusions, arguing that political ideology “prevented him from taking a detached view of the culture he was seeking to understand.” This criticism does not sit well with Bogue, who condemns it as a case of “intellectual patricide.” While this comment strikes me as considerably wide of the mark, it is clear nonetheless that Worster’s politics influence his history at least as much as Malin’s did his.<sup>20</sup>

In the tradition of DeVoto and Webb, Worster writes as an advocate as well as a historian, arguing that “the history of this region, if it wants to be vital and listened to, cannot be kept isolated from public controversy.” There are no politics in this work, however, perhaps because he seems not to envision change arising from the existing configuration of social and political forces in the West. For Worster, nature itself offers the only hope of real transformation. Pointing to the build-up of salinity in western rivers, the result of intensive irrigation, he warns that “nature has a way of undermining the most entrenched elites in history, especially when they have overreached themselves.” As Richard White observes,

<sup>19</sup> Worster, *Under Western Skies*, 15.

<sup>20</sup> Worster, *Under Western Skies*, 94–95; Bogue, “James C. Malin,” 233. See Richard White, *Land Use, Environment, and Social Change: The Shaping of Island County, Washington* (Seattle, Wash., 1980); William Cronon, *Nature’s Metropolis: Chicago and the Great West* (New York, 1991); and, for a selection of Malin’s work, James C. Malin, *History and Ecology: Studies of the Grassland*, Robert P. Swierenga, ed. (Lincoln, Neb., 1984).

Worster writes environmental tragedies in which the only "satisfaction we gain is the knowledge of our limits."<sup>21</sup>

The essay that concludes Worster's collection, however, finds him reflecting on some of the limitations of his own approach. "We do need a better story than the one we've been telling about the West," he now believes; "if nothing else than to save us from gloom and excessive pessimism." In the end, he suggests a historical study that seems to move closer to Malin's concerns: "We need a new past, one with the struggle for adaptation as its main narrative, one that regards successful adaptation as a kind of heroism too."<sup>22</sup>

THE THEME THAT SPARKS THE MOST DEBATE IN THESE ESSAYS is the matter of West and frontier considered as place and process. Worster counts himself among those arguing strongly for place, for a regional approach. He adheres to Webb's declaration that "the heart of the West is a desert, unqualified and absolute." "I know in my bones," writes Worster, "that Webb was right." All the West, of course, is not desert, but aridity has played a shaping role in the region as a whole. Consider, for instance, the impact of water-hungry southern California on the water-rich portion of that state. In Webb's phrase, the American West is an oasis civilization, but developers have often created the oases by diverting water from somewhere else. Other traits help define the region: the extraordinary control over land and resources exercised by the federal government, the continuing dependency on extractive industries with a resulting boom-and-bust economy, and the largest populations of Hispanics, Indians, and Asians in the country. Furthermore, as Michael P. Malone points out, "a major part of the region's shared cultural heritage lies in the drama and recency of the frontier's passing." To objections that such traits fail to add up to a coherent regional identity, Limerick responds sensibly that, "judged by equally stern standards, very few regions—and fewer nations—would qualify as genuine units of society or units of study."<sup>23</sup>

Problems arise not so much from the loose regional boundaries of today's West as from the contingency of the term "West" itself. Moving back through the American past, it refers to ever more eastward regions of the continent: to Arkansas, Missouri, and Iowa before the Civil War, when Americans considered the West of today to be "the Far West"; to the old Northwest and old Southwest territories at the end of the eighteenth century; to Kentucky and Tennessee during the American Revolution. Turner used the phrase "the Great West" to encompass them all. But the new western regionalists have no patience with such historically mobile definitions. "If 'the West' is sometimes in Massachusetts, sometimes in Florida, sometimes in Kentucky, sometimes in Illinois, sometimes in

<sup>21</sup> Worster, *Under Western Skies*, 15, 77; Richard White, "Trashing the Trails," in *Trails toward a New Western History*, 32.

<sup>22</sup> Worster, *Under Western Skies*, 253.

<sup>23</sup> Webb quoted in McGerr, "Is There a Twentieth-Century West?" 246; Worster, *Under Western Skies*, 24; Malone, "Beyond the Last Frontier," 150; Limerick, "Trails to Santa Fe," 70.



California, sometimes in Colorado," says Limerick, "then what on earth is a 'western American historian.'"<sup>24</sup>

Another contemporary group of western historians answers with the frontiers approach. By no means does regionalism command a consensus in these essays. Indeed, argue the editors of *Under an Open Sky*, the paradox of process and place is "the central problem" of western history. "The West may be the region lying somewhere beyond the Mississippi River, but it is also the experience of going there." In this view, western history is the study of "the Great West," with special attention to the "last" West in the vast and arid trans-Mississippi region, an approach that obviously owes a great deal to Turner. "We share his belief that a comparative study of parallel regional changes—'frontier processes'—has much to offer," they write, for "without it, regional history loses much of its broader significance." Where their model differs from Turner's is in the nature of those processes, which they characterize as a series of open-ended transformations carrying no imperial implications about progress.<sup>25</sup>

David J. Weber, a historian of the Southwest who recently served as president of the Western History Association, argues that "abandoning the idea of the frontier and making the West *as place* the center" threatens to drain away "the drama of life on the *edges* where people and places meet." Turner studied the frontier not merely because it was his special area of interest but first and foremost because he believed its history could illuminate the broader story of America. William Cronon believes that "to jettison Turner's frontier in favor of an apparently less problematic 'regional' definition runs the grave risk of abandoning the cross-regional and national emphasis he sought to establish for the field." His perspective reminds us that the power of Turner's frontier thesis derived from its commitment to the study of what it has meant to be American. That is part of the Turnerian view we would do well to preserve.<sup>26</sup>

<sup>24</sup> Patricia Nelson Limerick, "Making the Most of Words: Verbal Activity and Western America," in *Under an Open Sky*, 168.

<sup>25</sup> William Cronon, George Miles, and Jay Gitlin, "Becoming West: Toward a New Meaning for Western History," in *Under an Open Sky*, 26, 6.

<sup>26</sup> Weber's comment is in *The Legacy of Conquest*, by Patricia Nelson Limerick: A Panel of Appraisal," *Western Historical Quarterly*, 20 (1989): 317; Cronon, "Turner's First Stand," 93.

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## Featured Reviews

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JAROSLAV PELIKAN. *The Idea of the University: A Reexamination*. New Haven: Yale University Press. 1992. Pp. x, 238. \$30.00.

John Henry Cardinal Newman and his educational views have had a curious place in the intellectual history of American higher education. Semantic confusion set in almost immediately. In 1852, when *The Idea of a University* was published, Americans had no universities, either in Newman's sense or that conveyed by the term today. Cornell University's first president, Andrew Dickson White, aptly if self-servingly characterized the American educational scene in the 1850s (that is, pre-Cornell) as dominated by "a regime of petty sectarian colleges." When mid-nineteenth-century Americans thought of universities it was the new and intriguing German universities that came to mind, which were not at all what Newman had in mind.

Biography also accounts for the slowness of his American cousins to warm to Newman. That at forty-seven, after three decades as student, fellow, and leading intellectual light, he left Oxford in 1848 to convert to Roman Catholicism and enter its priesthood, won him little favor among contemporary American educators. Protestant to a man and seldom free of anti-popish sentiments, they were loathe to identify with arguments from *The Idea of a University* that Newman first presented in a series of lectures commissioned by the Bishop of Armagh in 1851 on the inauguration of the Catholic University of Dublin, over which he unhappily presided for seven years.

Accordingly, even when Newman's views squared with their own, contemporary American educators seldom cited him. Instead, for example, Yale's President Noah Porter, in *The American Colleges and the American Public* (1871), made do with his own arguments against the forces for reform beginning to muster at Harvard under its newly installed president, Charles William Eliot. To be sure, with the launching of the Johns Hopkins University in 1876, it would have taken more than the regular incantation of Cardinal Newman to stave off the rapid emergence of the American university in a form immediately recognizable today. Champions of the university in its

most Germanic form—Daniel Coit Gilman, G. Stanley Hall, Thorstein Veblen—simply ignored Newman. What is curious for present purposes is that those with second thoughts about the university revolution—William James, Irving Babbitt, even George Santayana—did too.

As often happens, Newman's ideas first entered substantially into the recurrent American debate over the purposes of higher education by way of someone fundamentally unsympathetic to them. If ever there was an unreconstructed advocate of the American university and its research ideal as it first emerged in Baltimore under President Gilman, it was Abraham Flexner, one of the first Ph.D.s produced by Hopkins. It was Flexner who, in his *Universities: American, English, German* (1930), in explicitly dissociating from both, linked the cause of the American college with Newman's ideas. In the decade that followed, as Chicago's President Robert Hutchins (in *The Higher Learning in America* [1936] and elsewhere) took on his own research-oriented faculty on behalf of undergraduate teaching and regularly invoked Newman, his place among the champions of "the collegiate ideal" was, if belatedly, rendered permanent.

To be sure, Newman's place in the academic litany of our time is often that of the obligatory reference, inserted by author or commencement speaker to certify bona fides, before getting to matters at hand. Nonetheless, when invoked for substantive polemical purpose, Newman is regularly enlisted on the side of the liberal arts college as against the research university, that of undergraduates against graduate students, and, perhaps most insistently, of "teaching" as against "research." Those to have done so of late include, among others, Ernest L. Boyer, Dinesh D'Souza, Page Smith, Charles J. Sykes, and Bruce Wilshire. As the ranks of those skeptical about the compatibility of research and teaching increase, so can we expect citations to the scholarly Cardinal.

These skeptics, whether champions of undergraduate teaching or critics of the research university,

have a point. One has only to get five lines into the preface of Newman's *Idea of a University* to find the following: "[The University's] object . . . is the diffusion and extension of knowledge rather than the advancement. If its object were scientific and philosophical discovery, I do not see why a University should have students."

What, then, is Jaroslav Pelikan, a Yale Sterling professor whose academic career has been spent at the University of Chicago and at Yale (where he has edited and authored some two dozen books), doing in "a sympathetic dialogue" with Newman? Again, biography provides a partial explanation. Pelikan finds in Newman a kindred spirit. For both, it holds what Newman said of himself: "From first to last . . . education has been my line." But more, they share the scholarly study of the early church fathers and the development of Christian doctrine; the faith of practicing albeit institutionally wary Christians (Pelikan's term is "believing scholars"); and a common experience with academic administration that left both concluding with Newman that they were "not fitted for the task of authority, or of rule, or of initiation." But the most relevant bond, and that which makes Pelikan's book so timely, is Pelikan's and Newman's shared willingness, even eagerness, to assume the role of defenders of their respective universities. For Newman that meant first responding to "the storm breaking upon the University" represented by the doctrines of the Scottish utilitarians, and then to Catholic critics who resisted his idea/ideal of a Catholic university whose object is "intellectual, not moral." For Pelikan it means taking on those engaged in "university-bashing."

Pelikan's use of Newman in his defense of the research university is strikingly comprehensive. Even when Newman's statements in *The Idea of a University* run contrary to Pelikan's own views—as in Newman's opposition to research as an appropriate function of a university—Pelikan attributes such apparent contradictions to context (for example, Newman's lack of familiarity with and suspicion of German universities)

or timing rather than to "first principles." More characteristically assuming the offensive, Pelikan argues that Newman's insistence on "knowledge as its own end" (that is, "independent of sequel"), when combined with the importance he places on teaching, renders him fundamentally sympathetic to the joining of research and teaching that for Pelikan defines the contemporary American university. Although "not directly derived from Newman," Pelikan nonetheless concludes that the inclusion within the university of professional schools, research libraries, and university presses (but not big-time athletics) "are fundamentally congenial to his outlook."

Besides recapturing Newman from the university-bashers, Pelikan's most important contribution to the current debate over academic priorities is to take on those "who would, in a laudable concern for the sake of the quality of teaching in the university, subordinate research to teaching." His arguments to the contrary are contained in two chapters that form the hinge on which his elegantly constructed book turns: "The Advancement of Knowledge through Research" and "The Extension of Knowledge through Teaching." Together they represent the strongest case I know of for the necessity of an active research program for all university teachers, and the crucial importance of regular teaching, especially of undergraduates, in the life of a university scholar. That the case for the scholar-teacher is made by one makes it all the more compelling.

What we have in Pelikan's book is the most informed, sustained, and unsentimentally argued defense of the American university now at hand. And none too soon. But it is more than an *apologia pro alma mater*. Pelikan has also provided us with an *apologia pro vita sua*, a stirring defense of the academic calling in which teaching and scholarship are joined, or what Newman characterized as "arduous, pleasant, and hopeful toil."

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LINDA COLLEY. *Britons: Forging the Nation 1707–1837*.  
New Haven: Yale University Press. 1992. Pp. x, 429.  
\$35.00.

"I will never, come hell or high water, let our distinctive British identity be lost in a federal Europe." Thus spoke John Major at his party's 1992 conference, in an address in fact aimed at convincing his fellow Conservatives to rally behind ratification of the treaty on European union. The origins of such equivocation are brilliantly told in Linda Colley's new book.

This work is an imaginative re-creation of "the identity, actions and ideas of those men and women who were willing to support the existing order" (p. 1) during the Georgian era. Colley does an end run

around the generation of revisionists who characterized an eighteenth century of industrial revolution and rising political radicalism in terms of strife rather than stability. Old-fashioned chronological parameters now reworked and the retelling of familiar stories with new insights are part of what makes the book satisfying. So does the complex analysis readers might expect from her earlier work on eighteenth-century Tories, on royalty, and on patriotism, and the lavish use of prints in the tradition of Simon Schama's *The Embarrassment of Riches* (1988), another book about a

Protestant nation convinced it was the new Israel. Colley's book is part of a growing body of research on the political culture of patriotism (one thinks of the work of Gerald Newman on English Francophobia). Colley also weaves into her story all those things general readers should know about one of the most stable yet rapidly changing eighteenth-century societies: religion, commerce, imperialism, political movements, the American Revolution, nobility and monarchy, women, and mobilization for war.

The evolution of Britishness came through two modes. First, there was the language of patriotism: national identity was superimposed on regional identities via Protestantism and wars with France. In some ways the greatest disappointment with the book is that Ireland is incompletely discussed, for the now quarter-century-long Ulster conflict lends credibility to her larger argument that a Protestant British identity rooted in the memory of Catholic assaults on British liberty came out especially in times of insecurity.

Second, there were the images of nationalism that shaped Britons' understanding of their character. A "vast superstructure of prejudice" (p. 36) reminded Britons who they were not—French or black—yet there was more to it than bigotry, as recaptured in particularly innovative uses of what British people saw and heard. In the case of John Wilkes, prints and songs stirred up the national memory. Wilkes became the personification of liberty, as liberty was the personification of the national character, so that a radical challenge was embraced within a traditional image.

The factors that fostered stability were closely connected to combat. On the one hand, the British never lost a war after the late seventeenth century, except in 1783. Defeat at the hands of the Americans led not to imperial disintegration but to clarification of the relationship between metropole and colonies, with acts of parliament concerning India in 1784, Canada in 1791, and Ireland in 1800. Colley notes a pattern of Britons discovering who they were while fighting Others: Americans, the French, or non-whites on colonial frontiers. On the other hand, there emerged a pattern of self-interest or even a cult of commerce that linked the landed elite and commercial community. Aristocrats could not afford to ignore the fact that much of the national debt was held by the middling sort, who in turn needed law and order at home and the Royal Navy abroad to protect their investments. This mutually beneficial relationship was evident in 1745, when the Jacobite army was defeated not so much by effective government forces but, even in Scotland, by some resistance and less enthusiasm, or what Colley calls active loyalty, most critically in manufacturing centers.

There followed in the succeeding two decades a broad debate about the boundaries and meanings of citizenship. Sometimes this found positive expression, as in the founding of the British Museum (1753) or the *Encyclopaedia Britannica* (1768), but at times it resulted in negative attacks on trade or aristocracy as

corrupting the nation. Patriotic societies founded in this era challenged the oligarchy and encouraged active patriotism on the part of the bourgeoisie.

These same years saw penetration of the political center by Scots, particularly via the imperial army, paralleling integration of English and Celtic elites through intermarriage and the transfer of land. Thus was created "a new unitary ruling class in place of those separate and specific landed establishments" (p. 161). A new-style monarchy was another crucial element. The public display of patriotism increasingly focused attention on the British monarchy during George III's long reign, particularly toward its end. Frequent tours by the younger royals and a rebuilt capital fostered a sort of secular religion and melodrama (or soap opera in our times) rolled up in one. If the nation's character was indeed transformed, then to a large degree this was because the top people were themselves reshaped.

Colley's most original verdicts are in the middle of her book, covering the years from the American Revolution to 1815. The first concerns the female face of patriotism. The true position of Georgian women was more complex than statute, custom, or moralists suggested. Women had a significant public presence—in elections, in patriotic activism, in petitioning—even if they were not citizens. The second concerns the mobilization of men to meet Napoleon's threatened invasion. Based on her analysis of Defence of the Realm returns of 1798 and 1803, created to answer the question, "Who was willing to fight for king and country?", Colley confronts those who assume that working-class Britons were a mass of deferential loyalists, as well as those who argue that the same men were Jacobins. Given regional differences, the most positive response to the call to volunteer for civil defense lay in areas where towns and commerce were important. Colley readily acknowledges that her findings were not simply the product of patriotism but of economic pressures, coercion, self-interest, and fear of invasion. Such men wanted to be asked, not forced, to serve their county or country, and they reserved the right to complain about—even riot against—internal repression.

The final phase in the forging of Britishness came during the two decades after Waterloo, in nationwide mobilizations over Catholic emancipation, parliamentary reform, and slavery. That all bore fruit between 1829 and 1833, given the authoritarian nature of early nineteenth-century British politics, was quite remarkable. Reformers as early as about 1810 were able to reappropriate the mantle of British patriotism lost to the Right in the 1790s; radicals won the battle of political language—or better, the Right lost it—for radicalism was indeed far more fragmented than Colley makes it out to be. The result was an act in 1832 that created in Britain one of the most democratic electorates in Europe. Yet here her larger thesis breaks down. As Colley herself suggests, results of legislation in 1832 were uneven in the four compo-

ment parts of the United Kingdom. If Britishness was connected to citizenship, Colley reminds us that not until 1884 were these parts reformed as a single unit.

Read for its insights into British political stability in a revolutionary era, this remarkable book's central contention cannot be contested: "The growing involvement in politics of men and women from the middling and working classes that characterised British society at this time was expressed as much if not more in support for the nation state, as it was in opposition to the men who governed it" (p. 372). But the factors that fostered political stability do not exactly correspond to those that made for Britishness. Although Colley pays great attention to religion, it may not have been enough. She argues that "Protestantism was the foundation that made the invention of Great Britain possible" (p. 54). True enough, but the argument that divisions between Protestants in the eighteenth century were less than those that divided Protestant and Catholic must be qualified. There was mutual loathing between eighteenth-century Anglicans and Dissenters. And was not the nineteenth-century High Church—Evangelical dichotomy equally divisive?

Providence replaced divine right in about 1700 for apologists of monarchy. Colley too often sees concepts like Providence as unchanging. But surely a

defensive eighteenth-century notion had changed significantly when, in the early nineteenth century, a Briton could imagine students writing essays about "The Probable Design of Divine Providence in subjecting so large a portion of Asia to the British dominion" (p. 170). In the more religiously pluralistic Britain after 1830, a purely Protestant Providence could not mean the same thing as it did in 1688 or even 1745. And what did such fixtures of Britishness mean for the hundreds of thousands of Catholic Irish who spent at least part of each year in England? Would the words of Northern Ireland's first prime minister in the 1920s, that Ulster was "a Protestant state for a Protestant people," promote *their* Britishness?

One final matter is the present-mindedness of the book. Colley makes the point that there is no longer an empire for ambitious younger sons—especially Scots—to loot or rule, and it is unlikely that marginally Protestant Britain will in the near future go to war with marginally Catholic France. So contemporary Britons have lost their way. But this is to miss the ambiguity with which this review began. The sense of incompleteness in the "forging" process was always there. Think of the nomenclature: in our *English* history courses, do we not have at least one lecture on the *British* empire?

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SIMON SCHAMA. *Dead Certainties (Unwarranted Speculations)*. New York: Alfred A. Knopf. 1992. Pp. 333. Cloth \$21.00, paper \$12.00.

An unconventional book such as this one by Simon Schama deserves or perhaps simply gets an unconventional review. This review takes the form of a dialogue between Luise White, an African historian, and Barbara Hanawalt, a medieval historian. They have both experimented with ways of presenting unusual evidence and agonized about whether or not the stories they tell represent historical realities. Hanawalt's new book, *Growing Up in the Middle Ages*, uses short stories to give life to disparate archival gleanings. The stories were based on real people who appeared in the records, but the stories were fleshed out with other evidence, creating a composite of experience. As a social historian, she has pulled together assorted information from reading many court rolls. This information is normally presented either in a quantitative or a case study narrative. Both methods are incomplete, however, because they never end with conclusions about what happened to a particularly gripping case or even to the average story. Hanawalt has thus used several different narrative techniques to explore the experience of childhood and adolescence in medieval London.

White has worked with oral histories that present coherent stories about vampires in colonial and mod-

ern Africa. To the modern, Western listener these seem fanciful. Her interest in narrative is to dissect the various parts of these tales. Why, for example, are firemen and specific orders of Catholic priests accused of sucking blood? What do these stories say about the colonial experience, confrontations with Western medicine, disruptions of migrant labor, and changes in gender roles? White's work takes factious, personal narratives and reconstructs them into social and economic history.

The reviewers, therefore, are keenly interested in how to tell a story. They are not insensitive to the problems of writing histories into which one places dialogues, motives, and premeditated actions. How much leeway does the historian have in filling in the missing evidence to tell a coherent story? What are the risks of allowing the historian's voice to intrude even more in the interpretation, if part of the story is made up? Because so much of writing history is interpretation in any case, Hanawalt and White believe that historians should boldly move beyond current conventions of historical expository writing and explore all avenues for presenting the story. Part of the tradition of writing history, a tradition which modern historians have lost, is to write for the enter-



tainment of the reader. For reviewers, the use of different writing approaches means developing different standards for reviewing a successful book.

To set the stage for the dialogue that follows, a summary of the book will be helpful. Schama divides his book into two distinct parts. The first half is an investigation of ways to portray the death of British Army General James Wolfe on the Heights of Abraham in 1759. Schama creates first a narrative account of the scaling of the Heights from the perspective of a hypothetical soldier's memoirs. A reconstruction of Wolfe's reflections on the strategic problems he faced follows. He then takes up the narrative of Wolfe's death through the exhibition of Benjamin West's famous picture at the Royal Academy in 1771. West's own smug description of how he came to paint the picture and his subsequent fame add to the interest of this section. Although an American, West was trained in Europe and pursued his career there. The next narrator, Francis Parkman, was a Bostonian with a particular interest in the forests and native peoples of America. He consciously sought to present an American voice in the writing of history. We learn much about his family, Harvard training, and intellectual milieu. Schama then brings his story back to the eyewitnesses on the battlefield.

The second part of the book seems to be related to the first only by the coincidence of Francis Parkman having a familial relationship to George Parkman, a murder victim. Schama does not make much of the relationship. In a full discussion of pre-Civil War Boston and its Brahmin life, we learn that George Parkman was a physician by training and had studied in Europe. He returned home to a life dominated by real estate and renting, although he kept a connection with medicine. In the course of collecting debts he disappeared and was presumed murdered. The accused, Professor John Webster of Harvard, was also a physician and had a passion for collecting fossils. After identifying these two characters, Schama continues with an account of the trial that eventually established Webster's guilt. Schama details at length the characters who appear on the witness stand, the lawyers for both sides, the judges, the crowd, and the governor. The reader is even treated to the participants' last letters and obituaries.

*Barbara Hanawalt (BH):* The book presents various versions of two violent deaths. The first is that of General Wolfe on the battlefield of the Heights of Abraham in 1759. The second is the alleged murder and dismemberment of George Parkman, a Boston doctor and creditor. The fragile bridge between the two deaths is Francis Parkman.

Schama's use of evidence has implications for both how historians use sources and for how they tell the story. But here there is a disjuncture. The sparse archival evidence recording Wolfe's death forced Schama to adopt a more classical style that has an elegance of the eighteenth century. The recourse to newspapers, letters, trial transcripts, memoirs, and so

on, makes the murder of George Parkman and the trial of Harvard Professor John Webster into an archival explosion and a narrative evoking Victorian excess. The disjuncture in the narrative is mirrored by a break in style.

*Louise White (LH):* I agree, but the reason is not just the amount of material available. This book makes a bigger deal out of the daily grind of conflicting accounts than many historians might: sources disagree and contradict or undermine each other. We all live with that, but many of us take what is in a funny way a less honest path—we mediate between them and come up with our own neutral narratives. Schama's unwillingness to diminish any voice or blur any point of view is important, but it does, in execution, lead to a whole bunch of voices holding forth. I was really excited by the section on Wolfe, however, especially the voices of Benjamin West and Francis Parkman.

*BH:* I agree that employing the different narrative perspectives, including that of Francis Parkman with his peculiarly American view of history, enriches our perception and understanding of what such a major event meant at the time. Even the creation of a common soldier's view adds a perspective that would be unavailable in that historical period. Schama's relaxation with inventing narrative and voices in this first part of the book is convincing.

*LH:* Not only is Schama telling us a story from the point of view of many participants but he is also writing about how such events are represented, in pictures and in words, and what those representations might mean at given times. But when this same sensibility was applied to a trial, with handbills and backgrounds and letters and who knows how many witnesses, I was less excited. Everyone who had a chance acquaintance with the deceased and the accused—dentists, janitors, lawyers—gets his own moment, his own voice, and his own relationship to the trial, so that we all but lose the murder in a cacophony of re-created voices. It becomes almost narrative for the sake of narrative: a good story, if you care deeply about the trial of John Webster, but not a good story if you are concerned about ways of reconstructing the past.

*BH:* The narratives surrounding the one historical moment of Wolfe's death tell the reader a great deal about what was happening to society and perceptions of art, taste, culture, national identity, relations with the Native Americans, and so on. The narrative of the trial, however, leaves mostly a memory for details and a few colorful characters, but it does not really give an idea of Bostonians of the mid-nineteenth century. What were their attitudes toward murder? Was the case mere sensationalism? What were their perceptions of class? After all, a janitor condemned a professor.

*LH:* But the janitor belongs to the only other class we see. Certainly he is the only working-class man who is allowed even to refer to his private life, his

wants, and his needs. The jailers have only their public selves. The cloying intensity of keeping up appearances in proper Bostonian circles comes across well, as do Webster's disturbingly disjointed moments, but Schama's reconfiguration of evidence to tell us about Webster is evidence not reconfigured to tell us about Boston. We end up with multiple readings of Webster, but nineteenth-century Boston Brahmin society is only a background for interpreting his life.

BH: The Webster/Parkman half of the book is disappointing. It lacks a focus for both the author and the reader. Are the characters more important than the history? What does the overabundance of detail tell the reader without the guiding hand of the historian? The narrative voice of the author is lost in the archival voices. The reader has no hint of the strength of one voice over another—instead the voices become a clamoring crowd. Historians learn one thing very quickly: "records do not speak for themselves."

LH: Oral historians also struggle with that issue; we brag that we "let the voices speak for themselves" yet we have already translated and edited them. But once we, or documentary historians, have deconstructed history into the voices of a time, each with its own authenticity, meaning, and right to be heard, we end up with many voices. Then the issue becomes presentation. How do we sort through the voices, decide which ones are to be heard and which submerged, and rearrange them to tell a story? Those decisions are what turns archives into stories, but Schama uses the abundance of evidence to turn stories into voices.

BH: In the case of George Parkman and John Webster, the archival materials are so rich that Schama's creativity, his historical imagination, is dulled. He loses his ability to pick and choose among the rich offerings of the Massachusetts Historical Society banquet. The trial is too long and undigested. Perhaps because his previous work has been in the early modern period, where the sources are scantier and require more interpretation, he became overwhelmed with the mass of material that modern historians learn to dismember.

LH: In some recent work, African scholars, primarily nonhistorians, have argued that the key to the way Westerners understand Africa is that it was imagined: it was a dark continent that could be enlightened by making up things about it. Africanists in the West, according to this theory, have simply

systematized and proved their hypotheses: all our work has been an exercise in historical imagination. I have ambivalent feelings about this line of reasoning, but I also like the way it emphasizes the role of imagination in scholarship—some things are "true" because we have worked incredibly hard, against all odds, to prove them. Schama seems to me to occupy the opposite position. Confronted with an enormous variety of accounts and speeches, he presents or represents them. His flair is awesome, but it also has cost him some of the pizzazz that we normally associate with scholarship: the rearrangement of unlikely details to come to an unpredictable conclusion. Schama's narrative self-confidence and breathtaking ability to master the idioms and anxieties of the times are quite wonderful, and they just dominate the trial. But it is these actors who speak, even in Schama's version of their voices, and the cost is that we lose Schama's voice.

BH: Medievalists seem to suffer, on the whole, from a failure of imagination in their approach to history. The scholastic tradition of sticking closely to the sources and meticulously interpreting them puts limits on imagining contexts for the sources. Some still believe that one can arrive at the *Truth* in history. Even writing in an engaging fashion that might attract the attention of more than a dozen other scholars makes medievalists nervous. Because of a certain lack of flexibility in my field, I appreciated Schama's willingness to throw caution to the winds.

LH: I agree. The idea and the effort of Schama's book provides almost a heady sense of the power and potential of enlivening written sources and constructing narratives. Craft is, as Schama writes in his afterword, the historian digging into the archives and picking and choosing voices to tell a story. Whatever my complaints about this particular book, Schama makes archival work look like great fun, and while the sources do not speak for themselves, they do speak with great clarity and passion.

BH: What I very much appreciate about Schama's book is that he has kept to the sense and accuracy of historical material while experimenting with different approaches to telling the historical narrative. I have always maintained that history is a damn hard way to write fiction.

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BLANCHE WIESEN COOK. *Eleanor Roosevelt*. Volume 1, 1884–1933. New York: Viking. 1992. Pp. xviii, 587. \$27.50.

In this comprehensively researched, generously written, and poignant work, Blanche Wiesen Cook places Eleanor Roosevelt (ER) at the center of her own life

and breaks open for us the complicated, difficult, and emotionally charged choices and events which constructed it. In its broad outlines, ER's life is well-

known. Both Roosevelt's autobiographical work and the biographical work of others have described the harshness of her mother's rejection, the aching separation from her loving but alcoholic father, and her heartbreak over his death when she was ten. Eleanor's life, we know, was redeemed in part by her father's affection and in part by the generous acceptance she found at the Allenwood school in France. Under the tutelage of Mlle. Souvestre, a teacher whose love was not merely indulgent but respectful, Eleanor found guidance unconstrained by the shallow conventionalities she encountered at home with her grandmother. Cook retells the story in her thoroughly engaging and confident style, describing afresh the beginnings of a sense of social responsibility for Eleanor in the settlement movement, the joy of her courtship and her early marriage to Franklin, the difficulties with her cold and controlling mother-in-law, and, then, the general crisis of Franklin's infidelity with Lucy Mercer, which left her certain that her dream of being completely loved was unattainable.

Cook's mastery of her subject permits her to paint Eleanor's youth in colors both deeper and brighter than before. Whereas Joseph Lash portrayed Roosevelt's girlhood as seamless unhappiness, Cook describes many early joys to balance her sorrows, most particularly in a new interpretation of her life with Grandmother Hall and her young aunts. "The Halls did everything possible to ease her pain and influence her life positively," says Cook. "The six years that Eleanor spent with her grandmother were a time of welcome constraint, and healing" (pp. 94-95).

But it is in the recasting of ER's life after the Mercer affair that Cook takes the story completely in hand. If Eleanor Roosevelt's biographers have heretofore sought the sources of her life in her grief, Cook seeks it in her passions. "In conventional terms, ER lived an outrageous life" (p. 14), Cook both asserts and demonstrates. By opening up ER's life to the question of romance and sexual passion outside her marriage, Cook transforms her from an icon representing perfect virtue to a heroine whose triumph over her disappointments, whose capacity for loving, makes her the more worthy of regard.

Cook's argument is that Roosevelt's intense connections to people had both public and private dimensions, and she challenges us not only to acknowledge them but also to confront the nexus between them. Historians, Cook says, have assumed that the women most admired have led the most sexless, desiccated private lives. "When looking at the lives of great women, we continue to divide the world into saints and sinners, and we make assumptions based on race and class, even looks. White, Protestant, aristocratic, and 'unattractive' women are not supposed to flourish in the political arena, and are not presumed to have sex or independently passionate interests. Regarding these women, all questions concerning that wondrous crossroads of sex and power have been traditionally

disallowed." (Cook asserts that the "issue of sex and power is assumed to be central to the lives of great men" but it is fair to ask if the role of sexuality has been well understood in writing the lives of "great men.") She claims that the relationship is cardinal. "What," she asks, "is energy and where does it come from? How do we channel energy—to write, to organize, to love? How do we acquire courage, develop vision, sustain power, create style?" Cook's answer is that great women are enabled by "their ability to express love and passion—to surround themselves with like-minded women and men who offered support, strength, and emotional armor" (p. 11). To support this contention, Cook speaks with authority about the nature of two of ER's relationships: her association with Earl Miller, the bodyguard assigned to her when FDR was governor of New York, and her affiliation with journalist Lorena Hickock.

Cook calls Miller "the first romantic involvement of ER's middle years" (p. 429). Tall and handsome, at thirty-two much the lady's man, Miller was twelve years ER's junior. His job made him her constant companion and Cook creatively employs photographic evidence to document his charm and protectiveness toward her, and the warmth of her response. While ER's affection for Miller is not in dispute, Cook brings to her portrait the rich detail that evokes what may indeed be referred to as passion. Cook draws back from the final conclusion that Roosevelt and Miller were physically intimate, in part because there are serious gaps in the evidence. Still, she strongly suggests that the relationship was physical, and she contends that the lack of evidence constitutes evidence itself. The friendship between ER and Miller, she declares, "remains an amazing study of denial and lost documents" (p. 435). But the case for physical intimacy between Miller and Roosevelt is thin not merely because the evidence is sparse; to believe it one must also believe that ER, who had been devastated by her own husband's infidelity, would have become involved in a relationship in which another woman (Earl's fiancée) was thereby the victim of betrayal and deceit. If ER underwent a personality transformation of that kind, Cook needs to explain it, and she does not.

Cook's discussion about the relationship between Hickock and ER is more compelling both because there is more evidence and because "Hick" offered the undivided love that ER craved. In the best known of their letters, Hick writes to Eleanor: "Most clearly I remember your eyes, with a kind of teasing smile in them, and the feeling of that soft spot just north-east of the corner of your mouth against my lips . . ." (ellipsis in original). The most provocative of the letters that Cook quotes from Eleanor reads: "I wish I could lie down beside you tonight & take you in my arms." Here Cook countenances not a scintilla of doubt: "They wrote to each other exactly what they meant to write" (p. 479).

These facts are of moment because Cook insists

that ER's specific sexual behavior is important to her argument about ER's public life. It is, however, a difficult link to establish. While love and passion may take sexual form, the equation is not ineluctable. And, in any case, Cook hangs on it more than she needs to. Whether or not ER went to bed with Earl Miller or Lorena Hickock, Cook's central understanding of her life is still convincing—ER lived an outrageous life; she drew "support, strength, and emotional armor" from her intimate friends and not, as she was supposed to, from her husband or her children. Cook shows that ER's life was outrageous because she insisted on defining her own boundaries, she forged intimate relationships outside her family, she sanctioned homosexual and unconventional sexual unions—whether or not she engaged in them. ER's choices continue to demonstrate that the lines that mark "acceptable" from "unacceptable" behavior betray a desire for simplicity and control that real life resists. She ignored those lines and loved both men and women, married and unmarried, of her "set" and outside it. We do not need to agree about her sexual life for our understanding to be enlarged by Cook's insight on this score.

Why was this outrageous crusader not a heroine to the new generation of feminists that emerged energetically in the late 1960s? Cook asserts ER's own feminism as she tells a detailed and richly painted account of Eleanor Roosevelt's meteoric career in New York state politics. Mindful at every moment of her place in the fray as a woman, careful never to underestimate the gifts and resources of the women around her or the reflexive sexism of the men, ER came into her own as a political power to be reckoned with. But ER's feminism was old style. She never failed to champion a public role for women and on equal terms with men, but she never relinquished the idea that women had unique and immutable responsibilities for children. Cook argues that feminists' indifference to ER lay in the "historical lie" that

cloaked her life, but perhaps the explanation lay in the truth about Eleanor's role as a powerful liberal leader and the consummate political insider, a reality that worked against her celebration by a movement of iconoclasts.

There is one surprising oversight in Cook's treatment of Roosevelt's relationships: she considers ER's troubled connection to her children almost parenthetically, introducing it primarily to explore the tension between ER and her mother-in-law. ER's children appear to have offered her little either in satisfaction or emotional involvement. When her daughter fights for a chance to go to college in the 1920s, ER is not her advocate. The capacious empathy that colors her later, passionate friendships seems to be virtually absent with respect to her children—until their own marriages. Cook offers little interpretation of whether or how this estrangement connected to the intensity of her other relationships.

The recent political season has been unique in its examination of potential "first ladies" and their endorsement of, or dissent from, the most conventional styles of life. "Eleanor Roosevelt," writes Cook, "remains a bellwether for our belief system . . . To this day, there is no agreement as to who Eleanor Roosevelt was, what she represented, or how she lived, her life" (p. 3). Cook's wonderful evocation of the early experience of this most complicated and esteemed of first ladies reveals how superficial our understanding has been of the relationship of public and private lives. Although Cook refrains in this volume from a clear statement of the connection between ER's private life and her public roles, presumably such an assessment will come in the second volume. That one, like this, will deserve a wide readership.

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RICKIE SOLINGER. *Wake Up Little Susie: Single Pregnancy and Race before Roe v. Wade*. New York: Routledge. 1992.  
Pp. xi, 324. \$25.00.

During the 1992 election campaign, Vice President Dan Quayle took television sitcom character Murphy Brown to task for her decision to bear a child out of wedlock. Describing the acceptance of unwed motherhood as part of a "poverty of values," Quayle argued, "It doesn't help matters when a character who supposedly epitomizes today's intelligent, highly paid professional woman is portrayed as mocking the importance of fathers, by bearing a child alone, and calling it just another 'lifestyle choice.'" As Rickie Solinger's book makes explicit, in his attack on Murphy Brown, Quayle drew on a deep historical tradi-

tion that has regarded unwed pregnancy and motherhood with fear and loathing. Solinger's account is a lucid and powerful assessment of the public meaning of out-of-wedlock pregnancy in the United States from 1940 to 1965. Her study breaks critical new ground in our understanding of unwed pregnancy in recent America and raises a series of fundamental questions that require further exploration.

The unwed mother has, as Solinger shows, been an important resource for the status quo—a symbol of pathology, degeneracy, and moral depravity; an argument for the powerful and catastrophic conse-



quences of sexuality outside of marriage. These meanings of illegitimacy shaped both public policy as well as the social and psychological experience of women who became pregnant outside of the marital bond. But as Solinger argues, the particular social constructions of unwed pregnancy have undergone significant shifts during the course of the twentieth century. Furthermore, as Solinger persuasively demonstrates, the problem of unwed pregnancy was fundamentally shaped by racial ideology. The interpretation and response to out-of-wedlock pregnancy among African-American and white women diverged fundamentally.

Among white women, by the 1940s and 1950s, the stigma and moral taint traditionally associated with illegitimacy came to be reserved exclusively for the mother; the bastard child, previously scorned, now became the valued commodity of an active adoption market in a child-centered culture. Prior to World War II, illegitimacy was a social marker that could not be reckoned with; the mother was "ruined," the child was generally viewed as the "offspring of infamy." This perspective was significantly influenced by hereditarian beliefs that emphasized the relationship of immoral sexual behavior to the genetics of feeble-mindedness. In the ameliorist culture of the 1940s, however, hereditarian explanatory frameworks of unwed pregnancy faded, especially in the characterization of white women. After the war, the nature and meaning of unwed pregnancy among white women underwent a transformation; now women who would agree to place the child for adoption could seek to be rehabilitated, "redemption through relinquishment." Although mothers were virtually required to keep their offspring in the past, now those who sought to were stigmatized and ostracized. Adoptions increased by some 80 percent between 1944 and 1955, a baby boom whose history has now been documented by Solinger.

Although the book is principally focused on the history of social policy and commentary on unwed pregnancy and illegitimacy, Solinger does analyze the workings of those institutions where pregnant girls were sent to relieve them and their families of the expected opprobrium associated with their pregnant status. Here girls would be isolated—sometimes under lock and key—awaiting the birth of a child who would generally be given up for adoption. Solinger persuasively suggests that these institutions, often sponsored by the Florence Crittenton Association of America or the Salvation Army, offered white girls and women a mixture of "protection and self-effacement." In institutions such as the Door of Hope (Jersey City) or Sheltering Arms Home (Philadelphia), pregnant women received rehabilitation and therapy, discipline and education in appropriate moral and family values. They were prepared for a radically different kind of domestic motherhood than their imminent unwed, institutionalized maternity implied.

With their offspring adopted they could, these institutions suggested, have another chance to return to the fold, to return from their extended visit to a "distant relative." Having identified a remarkable set of archival materials in which adolescent girls often described their experience in apt detail, Solinger unlocks the doors of these institutions. Within these walls, the women themselves created a cultural environment often at odds with the authority's emphasis on rectitude and training for domestic homemaking. Pregnancy itself could also be read as an act of opposition to dominant cultural expectations of sexuality, gender, and social role.

Solinger analyzes the prevailing explanatory discourse for the phenomenon of extramarital pregnancy among white women. Psychiatric and social work experts came to account for these pregnancies as markers of mental pathology. Rather than viewing out-of-wedlock pregnancy as a social or cultural phenomenon, it was defined as an individual medical problem, subject to therapy and institutional rehabilitation.

If white women were seen as individuals with "problems," unwed black women who became pregnant were seen in the aggregate, typical of their culture and race. Nonetheless, their pregnancies elicited considerable political wrath. Some southern lawmakers sponsored legislation to mandate the sterilization of women who bore some specified number of children. And unlike the emphasis on adoption among white mothers, among African Americans the prevailing expectation was that mothers should keep their babies. Although unwed pregnancy was in large measure viewed as an inevitable, indeed, "biological" aspect of black life, it was feared that liberal social policies such as Aid to Dependent Children (ADC) encouraged rather than discouraged the phenomenon. As public welfare costs increased in the postwar era, hostility was increasingly directed to the single black mother. The "welfare mother" became a new epithet in a growing debate about the social and economic costs of single motherhood. Social scientists and commentators invoked the "culture of poverty" and what they perceived to be deep-seated familial and sexual patterns to account for the decline of the black family. A Gallup Poll from 1963 found that 20 percent of Americans believed that unwed mothers should be mandatorily sterilized.

Changes in sexual practices during the early 1960s, in part accelerated by growing access to birth control, irrevocably altered the nature and meaning of unwed pregnancy according to Solinger. Out-of-wedlock pregnancies increasingly came to be identified as a technical failure rather than a psychological fault in a culture where premarital sexuality came to be a widely noted if not universally accepted practice. Although concern about the impact of illegitimacy on welfare costs and minority families continued to be heard, such cries were now often voiced within the



context of a growing debate about the availability of birth control and abortion.

Solinger is, of course, writing about an experience difficult to recapture historically. As she points out, maternity homes for unwed mothers served fewer than 10 percent of all such women during the post-war years. Unintended pregnancy was a dark secret, rarely discussed. The institutional record, including the debates about public policy and psychology, are but one critical aspect of this complex history.

In this sense, Solinger's book is about the public record of a private phenomenon. It is an intellectual and political history of a social experience. Greater attention, for example, to the nature of the sexual relationships that often resulted in out-of-wedlock pregnancy would have enhanced her study. Out-of-wedlock pregnancies underscored a persistent double standard regarding sexual initiation and expectations; remarkably absent from Solinger's account is an adequate assessment of the issue of nonconsensual sex as a historical phenomenon.

The significance of age in the assessment of extra-marital pregnancy merits greater attention as well. Were adolescent pregnancies accounted for in the same way as adult pregnancies? The discovery of "teenage pregnancy" suggests that our understanding of the question may well be tied to social notions of age. Questions of social class virtually disappear from her analysis, although it seems likely that socioeconomic status may well have had a significant impact on the experience of unwed pregnancy for both blacks and whites.

Finally, it would be useful to more fully understand the nature of perceptions and practice regarding unwed pregnancy and single motherhood among African-American women, but these voices are not heard in Solinger's study. This book is about the

public record and meanings of unwed pregnancy. The experience of such pregnancies can only be read through the public debate. Social control seems too restrictive an analytic lens to view the phenomenon of unwed pregnancy.

Absent from the book is any contemporary, self-conscious feminist critique of these ideas of single pregnancy. This provides but one important hint, perhaps, of how deeply insinuated within the culture these notions of the unwed mother have become. Nonetheless, further exploration of the ongoing debates about access to birth control, abortion, and sex education during the 1940s and 1950s might have provided an additional context for the debate about unwed pregnancy as well as an alternative "construction" of the problem.

One cannot help but note the deep persistence of the cultural views that Solinger so effectively delineates. In this respect, the book's somewhat abrupt conclusion in 1965 seems unjustified. Although the wider provision of birth control and access to abortion after *Roe v. Wade* did change the social debate about illegitimacy, there are powerful continuities. Despite the enhanced ability to prevent or terminate pregnancy, in American society the issues surrounding unintended pregnancy and single motherhood have remained a significant aspect of the continuing debate about teenage sexuality and gender. And yet, as Solinger makes clear in this important book, the issue has been discussed, debated, and analyzed in such a narrow cultural discourse that we have yet to develop an adequate vocabulary to address the phenomenon of unwed pregnancy that does not draw on the overtly sexist and racist notions of the past.

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FRANK GRAZIANO. *Divine Violence: Spectacle, Psychosexual-ity, and Radical Christianity in the Argentine "Dirty War."* Boulder, Colo.: Westview. 1992. Pp. xi, 328. Cloth \$49.95, paper \$18.95.

The Argentine military regime's systematic abuse of human rights and use of torture during the so-called "Dirty War" (1976-83) has so far largely avoided sustained scholarly reflection. The sheer horror of what happened has been effectively documented but more profound analyses of both causes and consequences have been scarce. Frank Graziano attempts to explore this terrain in his new book. Graziano's general purpose is to subject to a sustained deconstructive reading the multiple texts that constituted the military junta's discourse and that were embodied in narratives, rituals, and torture methods. Using such a strategy he argues that the junta's discourse was premised not on a hypocritical contradiction

between a formal rhetoric of messianic Christianity and a reality of torture and repression, but rather it was characterized by "a kind of rearranged truth, a mythologised reality in which the Junta's words and deeds were integrated components of a single, coherent agenda" (p. 8). For the military leadership, the "dirty war" was primarily a politico-religious experience; for the task forces it was a psychosexual one. Both, however, shared a common nexus and a corresponding "paradigmatic coherence."

Armed with a theoretical arsenal culled from Jacques Derrida, Jacques Lacan, symbolic anthropology, narrative theory, classic Greek mythology, and drama, Graziano searches for the "residuals" in the

junta's political and legal texts that betray the mythological/eschatological and psychosexual dimensions behind the formal legalistic register of the military's public discourse. For Graziano, the underlying dynamic informing the junta's actions was a primitive, medieval Christian apocalyptic vision with its corresponding eschatological mythology of Antichrist, redemption, punishment, and sacrifice. In chapters dealing with "The Strategic Theatrics of Atrocity," "The Mythological Structure of the Imaginary," "In the Name of the Father," and "Sacrifice and the Surrogate Victim," Graziano seeks to trace the embodiment and playing out of this mythology in the practices and rituals of the junta.

Although Graziano is to be commended for proposing to take us beyond functionalist explanations of Latin American military regime practices, there are major problems with this book that center on its neglect of any sense of historical agency and subjectivity and, in a related vein, in its hostility to notions of historical process, context, and evidentiary procedures. The real subjects of the book are the narratives, plots, and texts of the junta's discourse and their dominant textual devices, metaphors, tropes, and so on. Graziano constructs his argument largely by appealing to the textual logic implicit in these devices. An early example of what becomes a standard procedure is his analysis of the Spanish term *quirófano*—operating theater—used to refer to torture centers in Argentina. Seizing on the operating theater metaphor, with its notions of both performance and public viewing, Graziano constructs an argument concerning the "participant observer" status of the Argentine public. This may work as poetics (or theatrics), but Graziano consistently assumes, both here and elsewhere, a homology between texts and historical reality. In the case of the *quirófano* metaphor, the nexus between theater and real world is taken to be unproblematic rather than in need of construction. Hence, we find the direct assimilation in related passages of elements such as the audience (or chorus) for Athenian tragedies to the Argentine public of the late twentieth century.

The principal problem with this is that it precludes any attempt to understand the complex response of the Argentine public to military discourse and practice. The complicated spectrum of individual and group accommodation, denial, and resistance is never addressed. In its place, and by way of evidence, we find the constant reference to the refrain *algo habrán hecho, por algo será* (they must have done something/it must be for something), used to refer to victims of repression as if the simple enunciation of the refrain is sufficient to underpin an assertion of public complicity in the junta's spectacle of atrocity. This is an extremely thorny issue and not one that can be resolved by a convenient quote from Lacan to the effect that "To fail to recognize presupposes a recognition," or the invocation of Jean-Paul Sartre's concept of "bad faith," whereby it is asserted that the

Argentine public feigned "a role as uninformed" (p. 221). In some of these passages Graziano is dangerously close to blaming the victims and, at the very least, to being insensitive to the trauma and tragedy of everyday survival during the "dirty war."

Imbedded in Graziano's text is a profoundly ahistorical methodology. He roams anthropology and history at will to uncover direct analogies with contemporary Argentina that can be marshaled as proof for what are in fact no more than assertions. For example, he attempts to explain the symbolism involved in "disappearing" bodies by invoking headshrinker rituals practiced by Aguaruna Indians in the nineteenth century. Similarly, the last chapter on sacrificial surrogates is premised entirely on a direct analogy between Aztec sacrificial rituals and the Argentine detention centers. Used in this way, comparative, interdisciplinary examples cease to provide suggestive hypotheses and instead become ahistorical, naturalized abstractions. This is reflected in Graziano's consistent use of phrases such as "The Argentine public, like the Athenian audience . . ." (p. 81), and "In Dirty War Argentina as in Medieval Europe . . ." (p. 85).

For the historian the most troubling part of this is that there is no sense of the historical and material differences underlying such comparisons, there is no sense of contextualization or attention to historical particularity. It confirms one's suspicions about deconstruction's lack of concern for the real world outside the text, its antipathy to history, its preference for the synchronic. This book shows only the most tenuous concern for any foregrounding of its analysis in the context of Argentine history. In a book dedicated to the notion that a radical, medieval Christianity dominated the junta's discourse, we find no sustained attempt to situate such a claim within an examination either of the history of the Argentine military or of the historical development of Argentine catholicism. No alternative sources of military ideology are examined. Graziano asserts a direct line of descent from medieval Christianity to the ideology of the Argentine armed forces. We are told, for example, at one point that St. Augustine exerted a profound influence on the Argentine military mind, yet we are given no detailed proof for this assertion. The impression left by this sort of procedure is one of abstraction. In the end there is little that is specifically about Argentina in this book. Graziano is constructing (in order to deconstruct) a sort of ventriloquist script whose actors are like so many free-floating signifiers at the mercy of the author's theoretical whims.

In this regard perhaps the most problematic part of the book is chapter 4, "In the Name of the Father." Basing himself on Lacan's development of the Freudian theory of the Oedipus complex, Graziano interprets the psychosexual dynamics of the torture chamber in terms of the junta's (son's) offensive against subversives (the father) who were violating Argentina

(the mother). Using the *picana* (electric prod) the torturer/junta usurps the father's phallic power "through 'the act of love,' at the torture table" (p. 184). There is little clinical data on which to base this sort of analysis. Indeed, testimony taken from some torturers (the number and context are not given) about their sexual anxieties concerning their mothers is, apparently, the sole pretext for launching the Oedipal analysis. Yet once again the provision of detailed evidence is not really the point. The chapter is a sustained exercise in abstraction with the author's imaginative development, *ad absurdum*, of the logic of Lacanian theory. Graziano constantly assumes the legitimacy of a direct move from individual psychology to the psychoanalytical structure of groups and institutions, with no sense of the problematic nature of such a move. Much of what passes for evidence is in fact quotes from Lacan's texts with the word "junta" simply substituted for the original subject of the passages. There is also a curious gender blindness involved. The author seems to be unaware of the

difficulty involved in using his analysis of torture to explain the psychosexual dynamics involved in the torture of women. How are women "subversives" conceived of as the "father" threatening the "mother"?

Graziano has not been well served by his editors. There are some eloquent passages. The author at times demonstrates the nuanced touch of a subtle textual critic, as when he evokes the Kafkaesque logic of the junta's mythology, or the multiple significations of the term *desaparecido* and the power exercised by such a term over the Argentine public. There are, however, also all too many jargon-filled pages whose main purpose one suspects is a gratuitous display of erudition. It may well be that a deconstructionist/poststructuralist approach holds out the promise of real analytical insights into the dynamics of regimes such as the Argentine military junta. Unfortunately, this book does little to convince one of this potential. The promise remains unfulfilled.

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## Reviews of Books

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### GENERAL

KAREN OFFEN *et al.*, editors. *Writing Women's History: International Perspectives*. Bloomington: Indiana University Press, for the International Federation for Research in Women's History. 1991. Pp. xli, 552. Cloth \$37.50, paper \$17.50.

Most of the essays in this volume were prepared for an international conference on the history of women funded by the Rockefeller Foundation and held in Bellagio, Italy, in July 1989. There, representatives from nineteen countries discussed the internationalization of women's history as a field, presented developments within national boundaries, organized a network of historians of women, and addressed theoretical and historiographic issues. The representatives came from various national committees and two demographic history groups, all affiliated with the International Federation for Research in Women's History/Fédération International pour la Recherche de l'Histoire des Femmes (IFRWH/FIRHF). The IFRWH/FIRHF was founded in April 1987, and in September 1987 joined the International Committee of the Historical Sciences as an Internal Committee comparable to other areas of specialization. As an Internal Committee, the IFRWH/FIRHF was able to propose panels and participate officially in the 17th International Congress of the Historical Sciences in 1990. This book is one of several cross-cultural projects directed at broadening the field of women's history and creating concrete working relationships among historians from various countries.

The essays vary in approach, reflecting in part the varying levels of maturity of women's history as a field within different countries. Some authors report on the fledgling nature of their enterprise. For example, Mary Cullen writes of a small, face-to-face network of women's historians in Northern Ireland and the Republic of Ireland, where there are no universities with a professorship or lectureship in women's history and such courses as exist do not constitute part of the required curriculum. She identifies such areas as the study of sexuality and of life cycle as quite underdeveloped, with little attention paid to theory.

From several essays it is clear how the state of women's history is tied closely to challenges and

opportunities within the discipline of history as a whole. Writing of Italy, Paola Di Cori first identifies historical study as languishing, intellectuals as expected participants in political life, and the educational system as simultaneously "centralised and bureaucratised [and] . . . personalistic" (p. 445). In such a context, many of the exciting developments in women's history have occurred outside universities.

Similarly, in Spain, historians of women, like historians of other specialties, grapple with the legacy of Francoist historiography. The particular influence in women's history has been an emphasis "on the public arena of politics, suffrage and the labour movement" (p. 383).

In Nigeria, writes Bolanle Awe, women's historians struggle with the colonial legacy as well as the absence of women from traditional studies (including the UNESCO-published, eight-volume *General History of Africa*). Nigerian historians of women face severe problems of inadequate financial resources for universities generally and challenges in locating sources for documenting women's activity. They also seek to develop theoretical frameworks that explain African women's lives rather than to apply uncritically frameworks that derive from Euro-North American realities.

Ruth Roach Pierson's essay on Canadian women's history echoes this point by way of identifying the diverse historical experiences of immigrants and native Canadian populations. In addition, she warns of the tendency of scholars of the U.S. experience to assume the comparability of Canadian women's lives to those of U.S. women.

The global array of essays challenges assumptions about the advanced position of Western European scholarship compared with that outside Europe and North America. The essays on Austria by Brigitte Mazohl-Wallnig and Switzerland by Regina Wecker report relatively late and meager development of studies of the history of women. The tardiness is linked to the slow expansion of higher education in Austria and consequent small number of female faculty. Similarly, in Switzerland, with relatively few female faculty (much less female faculty interested in women's history), the pioneering history of women's suffrage was written by a historian/suffragist working

"outside the mainstream of academic research" (p. 355). In contrast, according to Noriyo Hayakawa, Japan had significant, synthetic efforts in writing women's history in the 1940s and 1950s; as a result, interpretive debates have emerged within the field of Japanese women's history.

Depending on the national educational, social, and political context, women's history initiatives may thrive inside academic institutions (Nigeria and Spain, for example) or outside them (as in Switzerland and Italy), or simultaneously in both arenas (the Netherlands). The preunification essays on the Federal Republic of Germany and the German Democratic Republic reveal interesting differences in ideology and emphasis; one wonders how the exciting East German initiatives have fared after unification.

In nationally based essays and in thematic and conceptual essays, issues recur. Theoretical debates around mainstreaming/integration versus focusing on women's history (or an "autonomous, feminist tradition" in the words of Dutch author Francisca de Haan [p. 266]) are under way in countries that have a longer and fuller historiographic development (for example, Sweden). Other familiar questions are those of pursuing gender as opposed to women as the object of study; equality/women's rights versus difference/women's culture as emphases; diversity and difference within the category of women (the Netherlands, India); the meaning of "feminism" as a term and concept (Spain). Women's historians in the Scandinavian countries who are writing a global history of women for a Scandinavian audience write of the problems of finding (or not finding) a unifying theoretical framework for such an endeavor.

In a brief review it is impossible to capture the diversity of these international developments in women's history while representing the commonalities; collectively the essays do both. This volume is essential for understanding the broad range of what is happening in women's history; at the same time it underscores the essential, if complex, link between the status of women practitioners in the discipline of history, the field of women's history as an intellectual endeavor, and the activities of women as activists to improve the conditions of their lives.

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HORST WALTER BLANKE. *Historiographieggeschichte als Historik*. (Fundamenta Historica, number 3.) Stuttgart-Bad Cannstatt: Frommann-Holzboog. 1991. Pp. 809. DM 264.

The title of this book reveals Horst Walter Blanke's program: to present a history of historiography not shaped by the narrative form, not restricted to a sequence of great texts or historians, but governed by the development of historical theory. The term *His-*

*torik* is an old one for the systematic reflection on the theory of history, last used more than a century ago, by Johann Gustav Droysen. The governing influences on Blanke's analysis come from his mentor, Jörn Rüsen, whose model for a matrix of a scientific history is linked to Droysen's *Historik*, and from Thomas S. Kuhn, whose concept of paradigms and the mechanics of paradigm changes provide key elements. These two explanatory models shape Blanke's ambitious attempt to reconstruct the paradigmatic realizations of the matrix of the historical science, to show how historians have reflected on the *Historik*, and to relate a history of historiography on that basis.

The author deals with the period since the Enlightenment—taken as the beginning of the *Verwissenschaftlichung* of history—and concentrates nearly exclusively on German developments, which for him are in advance of others in the field of *Historik*. He brings to bear his considerable expertise gathered in a multiyear collective research project on the problem of *Historismus*. His previous publications include articles and the co-editorship of *Von der Aufklärung zum Historismus* (1984), *Theoretiker der deutschen Aufklärungsgeschichte* (2 vols., 1990), and *Transformation des Historismus* (1991).

Blanke portrays the development of a historical science, firm in its theoretical foundations, as the sequence of three paradigms: the long dominant humanistic-rhetorical historiography of the *ars historica* was followed consecutively by the *Aufklärungshistorie* (Enlightenment historiography), *Historismus* (historicism), and *historische Sozialwissenschaft* (historical social science). In each case Blanke discusses the changes in symbols, values, and models and how the new paradigm maintained the link to life (*Tatsachenskonformität*), avoided contradictions, extended the range of explorations, and produced insights. In contrast to Kuhn's accent on the discontinuity between paradigms, Blanke blunts the sharpness of transitions by speaking of a gradual transformation process that carried over elements from paradigm to paradigm. The result is an account that is clear in its intentions and offers a wealth of information and insights on the three chosen paradigms; even on the humanistic-rhetorical historiography (the *ars historica* of lessons for life), which Blanke uses primarily as the "prescientific" foil for the *Aufklärungshistorie*. The latter emerges as the phase in which the issues connected with the *Verwissenschaftlichung* of historiography (making it a science) were grasped and partially responded to. The section on *Historismus*—defined as a paradigm marked by an individualizing approach, *Verstehen* (empathetic and critical understanding), and an emphasis on ideas and the nation-state—emerges neither as a mere cover-up for relativism nor as the ultimate peak of historical consciousness. Blanke finds the true successor paradigm to *Historismus* not in any of the attempts to modernize German historiography since 1880 (not even that of Karl



Lamprecht) but in *historische Sozialwissenschaft*. Although he considers it still in a state of flux, Blanke credits this school of historiography—in which Hans-Ulrich Wehler figures prominently—with an appreciation of social history, an encompassing range, a wholesome ideological criticism, but most of all an understanding for the need for theory.

Critics will find ample opportunities for disagreements in this book that tries to pioneer a new approach. Apart from specific reservations, the book will call forth those who would wish for a greater interdisciplinary emphasis in a story of historiography as a scientific discipline. Even more critics will insist on a tighter dependence of historiography on its social and economic contexts. For them, Blanke's story of historiography as the transformation of history into a scientific enterprise will be insufficiently cognizant of the "external" connections, although Blanke emphasizes the importance of "background and connection" as well as the emancipatory aspect of modern historiography. Nevertheless, the work suggests a special dynamic in the world of thought, one without a relapse into the old *Ideengeschichte* (the history of autonomous ideas). In terms of Rüsen's model, Blanke has deliberately remained mostly within one-half of history's matrix, that of the *Fachwissenschaft* (history as scientific discipline), with its theories, methods, and forms of presentation, while only referring to the other half, the *Lebenspraxis*, with its orienting interests and functions. For comparisons the reader has available other works, such as those by Peter H. Reill (on the historiography of the *Aufklärung*) and by Georg G. Iggers (on *Historismus*). No criticism, however, will diminish the status of the volume as an important and sophisticated attempt to deal with German historiography in the contemporary spirit with its preference for *Historik*. Although not intended by Blanke, the volume's wealth of factual and bibliographical information will also lead to its use as a reliable handbook.

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G. S. ROUSSEAU. *Enlightenment Crossings: Pre- and Post-Modern Discourses; Anthropological*. New York: Manchester University Press; distributed by St. Martin's. 1991. Pp. xv, 259.

This volume is the first in a three-volume set of G. S. Rousseau's collected historical and literary-historical essays issued under the collective title *Pre- and Post-Modern Discourses*. As the set's title ironically suggests, these essays span a range of time, taking the reader along a path leading to a view of postmodern academic culture; they also address historiographical and professional issues linked to the development of Rousseau's particular viewpoint and his sympathetic critique of several schools of contemporary scholarship concerned with the Enlightenment. The linkages

evident in this volume confirm the author's judgment that his condition has been symptomatic of an age, but on occasion Rousseau too self-consciously puts himself out on a methodological and discursive limb. I suspect that some readers may be put off by the self-referential nature of Rousseau's disciplinary "crossings," or by his strident multi-, cross-, and post-disciplinary strictures. Those who read this volume from start to finish, however, will find that a coherent set of issues emerges in parallel with the incremental transformations Rousseau documents.

None of the ten essays in this volume are published here for the first time. They originally appeared in journals or collections between 1969 and 1990. The author, serving also as his own editor, has changed some titles, occasionally (and incompletely) added a few recent references or updated old ones in the footnotes, and provided a subject and name index covering all the essays in this volume. These additions are of minor value and by themselves do not justify this compilation and re-publication of widely available articles.

The construction and chronological organization of this volume invite a reading of the articles in sequence, even if several may already be familiar to the reader. Few historians of science, for example, will not know Rousseau's article on the historiography of eighteenth-century psychology, first published in *The Ferment of Knowledge* (1980); here it is given a quick face-lift, confined to its jazzed-up title, "The Discourses of Psyche." Fortunately, Rousseau has provided more substantial reasons for consulting this volume than title changes: particularly, in addition to a general introduction, the bio-historiographical notes that introduce each essay. By striking the right chord of academic self-criticism, reminiscence, and contextual background, he underlines the unifying theme that the articles by themselves only suggest: the path of an academic career shaped in large part by milieu, but also flavored by personal outlook. Rousseau describes his work as unabashedly interdisciplinary and vows that "the commensurability of theory and history defines and forms the heart of my own pluralism" (p. 175). The biographical notes underscore this conviction by linking selected circumstances of Rousseau's career to his growth as a scholar and the transformations of his theoretical outlook.

The chronological sequence of the essays also documents some of the ways in which Michel Foucault and Thomas Kuhn, among other writers, shaped Rousseau's eclectic but focused mapping of the Enlightenment's "social anthropology" (a phrase appearing in two essay titles and throughout the volume). He takes care to tell us, for example, when he read certain of Foucault's key texts and how his own response to them took shape alongside their reception by other scholars. The focus of the individual essays in this volume is generally either methodological or topical; for the most part they are centered on Rousseau's resolute conjoining of literary, cultural,

and scientific subject matter. For historians or historians of science reading these essays in 1993, the most enlightening will probably be those that explore the impact of Foucault, delve into New Historicism, or lay out his linkage of Enlightenment physiology, philosophy, literature, and the mind-body problem. The more empirical essays, notably "Le Cat and the Physiology of Negroes" and "Madame Chimpanzee," seem by comparison rather thin and stand to the side of the main trajectory of this compilation. At its best, this volume provides an intriguing stab at postdisciplinary *Bildungsgeschichte*.

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MARGARET A. ROSE. *The Post-Modern and the Post-Industrial: A Critical Analysis*. New York: Cambridge University Press. 1991. Pp. xiv, 317.

This study by Margaret A. Rose presents itself, somewhat disingenuously, as a clarifying survey or overview of the confusing variety of theories of the postmodern and postindustrial articulated over the past two decades. There is a certain encyclopedic quality to the text, which bristles with one-dimensional, usually undeveloped, listings of various theory components, and it is often difficult to distinguish from the notes (105 pages, compared with 180 pages of text), which the author claims "should also be regarded as important as the text itself" (p. 181).

Rose begins by summarizing the historical evolution of the use of the central terms, then focuses on delivering descriptive synopses of "deconstructionist" and "double-coded" theories of the postmodern. She ends by noting the leftovers ("alternative theories") and providing a chronological recapitulation and overview of major contributions to the contemporary theoretical debate. Running through the lists, summaries, and surveys of both the text and the notes, however, there is an implicit but easily discernable polemical thread. Rose's intent is, on the one hand, to undermine and discredit theories that identify the style or period concept of postmodern with the theoretical positions of poststructuralism or deconstruction, or that theorize the postmodern from a post-structuralist or deconstructionist position. On the other hand, Rose wants to defend the neoconservative positions of Daniel Bell and especially the architectural historian Charles Jencks as representatives of a "good" postmodernism worthy of our intellectual acceptance and moral commitment. For all its encyclopedic pretensions, this is a book written about, for, and from the perspective of Jencks. His theories represent the "double-coded theory" of postmodernism that Rose advocates as convincing and morally correct, and everything else circles around, highlights, and supports this central focus.

The promise of the title—that Rose will seriously examine conceptualizations of the relation between

society and culture, between postindustrial technology, productive relations, and social relations, and postmodern popular culture, art, and theory—is not fulfilled. The problem of postindustrialism as a fundamental rearrangement of the conditions and relations that practically define what it means to be human is essentially dropped after a short chapter that chronicles the use of the term up to and including the works of Bell. Rose's aim seems to be to present postindustrialism as a social aggregate tending to produce, though not to determine, the dissolution of historical continuity, of the responsibility of the individual subject and of collective ethical and communicative norms. "Deconstructionist" theories mirror and reenforce the destructive, "nihilistic" aspects of this development. Both extend and exaggerate the contradictions of industrialism/modernism. Jencks is seen as responding to Bell's moral and cultural concerns with his emphasis on a double-coding of cultural frameworks in which the avant-garde extremism and purism of late modernism is rejected in favor of an amalgamation of modernist techniques with a return to historical traditions, the production of artistic signifiers that communicate beyond the expert elites, and the situating of buildings within specific natural and cultural contexts.

This defense of Jencks would undoubtedly be more convincing had Rose seriously addressed the deconstructionist analysis of the postmodern denaturing of subjective identity, historical continuity, and communicative context. It also would have been clearer had she paid some attention to disciplinary and genre differences, and set deconstructionists against new historicists within architecture, literary theory, and social philosophy, before opposing new historicism in architecture to deconstruction in literature and philosophy. Neither deconstruction in architecture nor new historicism in the human sciences finds a significant place in this peculiarly narrow "survey" of postmodern theory and cultural practice.

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ROBERT N. PROCTOR. *Value-Free Science? Purity and Power in Modern Knowledge*. Cambridge: Harvard University Press. 1991. Pp. xi, 331. \$34.95.

Robert N. Proctor presents himself as an activist scholar, and his ambitious book is typical of a sea-change in which many ethically engaged scholars have participated in recent years: from an emphasis on the social to the political context of science. Rather than asking how knowledge originates in a social matrix, Proctor asks why we choose to know certain things and not others, who wins and who loses by these choices, and what alternative ways of knowing we should consider. Given these concerns, the notion of value-free science is problematic to Proctor, and

this book is an investigation into its history from Plato to the present.

Proctor handles the formidable problems of coverage posed by such a project quite skillfully: rather than trying to be encyclopedic, he focuses in zoom-lens fashion on certain thinkers and movements within his panorama. Thus, Bacon and Hume receive more extensive treatment than Descartes or Locke; the middle part of the book deals largely with the *Werturteilsstreit* in German social science at the turn of the century; subsequent chapters deal with logical positivism, marginalist economics, the sociology of science, and the Edinburgh school.

This does not mean that Proctor gets all the details straight. There are a number of wrong names; one can find other faults depending on one's area of expertise. On German social science, for example, Proctor gives an overly restrictive depiction of neo-Kantianism in the third quarter of the nineteenth century; there is also a lack of appreciation of the diversity of views on neutrality and objectivity within the Social Policy Association before the German Sociological Society broke away from it. Most seriously, he neglects the subsequent use of the term "value freedom" within the debates of the Sociological Society itself. That use was notoriously imprecise, so that liberals, racists, and Marxists could each claim they were pursuing "value-free" science while accusing the others of not doing so. One person's fact was another's value.

Nevertheless, this is a book where the whole is greater than the sum of its parts; the lapses of detail rarely invalidate the larger points that Proctor makes. This book is far from being a political tract or a monolithic indictment of irresponsible science. Proctor is self-conscious about his methodology: he seeks to combine the opposed principles of sympathy and critique, that is, of understanding the past together with a sense of the missed alternatives that have stemmed from it. In the name of sympathy, he portrays the variety of meanings that the term "value-freedom" has acquired in different situations, its "geography," as he calls it (p. x). Neutrality in the seventeenth century was the product of a rationalist world view; in the fascist era it became a positive commitment to keeping science free from political influence; in the present it can be a convenient mask for not facing the ethical implications of one's professional activity. Proctor further distinguishes between value neutrality and objectivity. The former has to do with taking a stand, the latter with reliability of results. Scientific research can be objective and still serve a particular interest. In the name of critique, Proctor points to cases where science has excluded groups or options from its activity: Plato and Tönnies, for example, appealed to the purity of science as grounds for excluding women from it. In one chapter, Proctor provides a useful survey of the critiques

of contemporary science in matters of medicine, agriculture, biological determinism, and militarization.

I am left wondering, however, whether Proctor has not aimed his critical batteries at the wrong target. Is value neutrality, with all its elasticity, really at the root of the narrowness and lack of social commitment that troubles Proctor? Is it not rather that nexus of methodology and sociology that Thomas Kuhn portrayed in *The Structure of Scientific Revolutions* (1962)? Does not the puzzle-solving nature of normal science by definition exclude a wide-ranging sense of alternatives? And are not the problems of politicized science that Proctor raises really problems for political science? Is it not a question of viewing the scientific community as one party within a plurality of interests and perspectives—including those whose sense of social responsibility may be at odds with Proctor's—that needs to be adjudicated? Perhaps we could take a loose leaf from that utopian dreamer Saint-Simon, whose technologized version of checks and balances called for a tricameral legislature: one chamber for planners and visionaries, another for scientists, and a third for movers and shakers.

DAVID LINDENFELD

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TOM ROCKMORE. *On Heidegger's Nazism and Philosophy*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xi, 382. \$45.00.

This book is a useful contribution to the recent debate over the degree to which Martin Heidegger's philosophy is implicated in his partisanship for the Nazi dictatorship in 1933. It is very well researched. Tom Rockmore includes valuable summaries of the major German and French debates (both historical and contemporary) over the extent and significance of Heidegger's Nazism, performing an important service for an American readership unable to follow these debates in the original languages.

Rockmore seeks to answer two essential questions: First, what were the factors that motivated Heidegger's fervid, if brief, involvement in the Nazi cause in 1933–34? Second, did Heidegger the thinker ever truly renounce the intellectual bases of Nazism, even though he ceased to be an active participant in the movement after 1934 (when he resigned his post as Rector-Führer at Freiburg University)? On both counts, Rockmore properly counters Heidegger apologists who claim that the master's philosophy emerges unscathed from the National Socialist debacle. He shows how Heidegger's partisanship for Nazism was very much a "philosophical choice"; that is, it was predicated on the basis of several concepts in *Being and Time* (authenticity, resolve, destiny, and so forth). Yet this thesis has already been set forth in detail by

others (see, for example, Karl Lowith's classic essay, "The Political Implications of Heidegger's Existentialism" [1945], as well as my recent book, *The Politics of Being: The Political Thought of Martin Heidegger* [1990]). Moreover, the filiations between Heidegger's philosophy and political choice are often merely suggested and hinted at, rather than cogently demonstrated.

The book's more controversial claim concerns question two: that, to the end of his days, Heidegger never abandoned Nazism or something like Nazism. Superficially at least, this contention bears similarities to Victor Farias's claim in *Heidegger and Nazism* (1990) that Heideggerianism equals Nazism, unremittingly and through and through. But such arguments form the weakest part of Rockmore's book. They appear, moreover, in the context of lengthy discussions of Heidegger's Nietzsche lectures, his *Contributions to Philosophy* (1936–38), and his later critique of technology, in which the central theme of the book seems for long sections to have fallen from view.

Rockmore's position is that whereas Heidegger abandoned the inferior philosophical basis for Nazism purveyed by the regime, grounded in categories of biology and race, he continued to support National Socialism as a political movement. Or, as Rockmore argues repeatedly, "Heidegger does not reject National Socialism as such . . . and certainly [did not reject] the practical consequences to which it led; rather he objects to it for its theoretical deficiencies from his ontological vantage point, for its supposed failure to provide an adequate theory of Being" (p. 200).

But this is far from the case. Heidegger rejected "really existing" National Socialism with his resignation from the rectorship in 1934 and his subsequent refusal to support the regime actively. Contra Rockmore, this rejection of National Socialism was both philosophical and political. In Heidegger's view, Nazism's intellectual inadequacies ("racial thinking") determined its practical deficiencies. It failed to realize the authentic spiritual mission of the German nation and thus became just another instance of modern nation-state building and power politics. (Here there is a superficial parallel with Nietzsche's critique of the Second Empire.) The issue remains murky since Heidegger, in keeping with his characteristic philosophical arrogance, never came forward with an unequivocal public renunciation of the regime and all it stood for.

In 1935, Heidegger distinguished between "the inner truth and greatness of National Socialism" (*Introduction to Metaphysics*) as opposed to its debased historical reality. This distinction remained definitive for Heidegger's relation to the movement from this point hence. As others have pointed out, Heidegger maintained an allegiance to an idealized image of what National Socialism might have been had it proved adequate to the momentous "encounter be-

tween global technology and modern man" prophesied by Ernst Jünger (*Der Arbeiter* [1932]). Instead, the movement was waylaid by small-minded men and women. But does this mean, as Rockmore claims, that Heidegger never "breaks with Nazism on a political plane," that is, with Nazism as a political reality (p. 201)? This view is demonstrably untenable, for the idealized National Socialism that Heidegger had in mind (as disturbing as such a thought may be) was in fact predicated on a rejection of Nazism as a historical reality.

And here we come to the fundamental confusion at the heart of Rockmore's argument. In support of his claim that Heidegger never essentially broke with National Socialism, Rockmore (correctly) points to the persistence of *völkisch* motifs in Heidegger's later thought. He also continually highlights the fact that Heidegger never relinquished the idea that a realization of "the destiny of the German Volk" (nation or people) was a desirable end (pp. 282 and following). Both of these points are well taken. I would go along with the author in claiming that they indicate major shortfalls—historical, political, and conceptual—in Heidegger's later philosophy. They may even be positions that he continued to share in certain crucial respects with historical Nazism. But taken by themselves and strictly speaking, these are not "Nazi positions" and allegiance to them does not mean one is a Nazi. In the course of German history (going back at least to Johann Herder, Friedrich Hölderlin, and J. G. Fichte), many writers and political groupings have embraced the imperative concept of the German Volk and its historical mission. But that does not necessarily make them Nazis or even proto-Nazis. To wit: one might examine the case of such "conservative revolutionaries" (with whom Heidegger had much more in common than the National Socialists proper) as Oswald Spengler, Edgar Jung, or Ernst Jünger. Here we have instances of avowed fascists who were nevertheless non-Nazis (Jung was himself executed by the Nazis during the Röhm purge). These important historical and conceptual distinctions need to be preserved so that one is able to differentiate between nationalists, reactionaries, conservative revolutionaries, fascism (the Italian and French varieties), and National Socialism.

The philosophy of the later Heidegger may be indigent and even abhorrent for a number of reasons, many of which Rockmore has recounted well. But to render such claims plausible does not mean he must be tarred and feathered with the charge of "unregenerate Nazi." What we do know is that Heidegger never internalized the historical lessons concerning the folly of the German *Sonderweg*. And the evidence of this failure to learn, which abounds, is damning enough.

RICHARD WOLIN  
Rice University



PETER MASON. *Deconstructing America: Representations of the Other*. New York: Routledge. 1990. Pp. vii, 216.

STEPHEN GREENBLATT. *Marvelous Possessions: The Wonder of the New World*. Chicago: University of Chicago Press. 1991. Pp. ix, 202. \$24.95.

It is a sign of our times and of the salutary condition of history as a discipline that these two books should be reviewed in the *AHR*, and reviewed by a nonhistorian at that. One of the authors is an anthropologist (Peter Mason), the other an English professor (Stephen Greenblatt), while I am a professor of comparative literature. The subject of these two books is the cross-disciplinary field and its potential for yielding insights that might have escaped the focus of traditional historiography. Mason's book, by far the more rigorous of the two, applies to the historical field the methods of ethno-anthropology, while Greenblatt's volume uses the methods of cultural criticism and, specifically, what has come to be identified, along with Greenblatt's earlier work on Shakespeare and the English Renaissance, as neo-historicist literary scholarship. Either book could well prove irksome to the mainstream historian. The language is a hybrid discourse, and the procedures are quite divergent from the usual historical narrative. Mason engages in a highly technical terminology and employs impressively arrayed data of an anthropological and historical sort. Greenblatt finds himself somewhat involuted in a self-reflective discourse of anecdote and interpolation. A classicist with a doctorate in anthropology, Mason systematically engages an overwhelming range of material relating to cultural estrangement through imaginative monstrosity and socio-physiological aberration projected on the unknown. Greenblatt sets out to string a series of anecdotes related to John Mandeville, Christopher Columbus, and Hernán Cortés, among other Europeans, who found themselves at the outer edges of known geography, imaginary or otherwise. His purpose is to show how possession or incorporation of the other has certain prerequisite rituals that are culturally and ideologically determined, in this case the mechanisms of marveling and wonderment. Having written a more descriptive and less ambitious book, Mason accomplishes a good deal more, while Greenblatt aims to disclose certain patterns of cultural interaction in order to substantiate a thesis with regard to marvels, particularly the wonders of the New World, and the way in which these served the ends of conquest and the appropriation of the strange into European familiarity and colonial hegemony.

Both authors argue that we project on the alien an otherness that we subsequently assimilate, with Mason showing how the New World is both a mirror that reflects back our projections as well as an opaque reality with its own aberrant strangeness that finds its object in the invading aliens and their projected nightmares. Mason yokes suggestively the philosoph-

ical reflections on otherness in contemporary thinkers, such as Levinas, with the "natural" science of Pliny and the Plinian races of European lore. He begins by examining the otherness within European culture itself and examines how this obsession with the aberrant finds its target outside Europe in the epoch of "discovery." The title of Mason's book could be misleading to the uninitiated into the discourse of current theory since *Deconstructing America* is really more about the deconstruction of the language with which America has been represented, that is to say, the language of Europe. Alternately, Mason goes to some pains to reflect on the nature of ethnocentrism in describing the other, speculating that perhaps the condition is not unique or peculiar to Europe. Greenblatt also suggests this possibility, but he notes the obvious sparseness of the archival record regarding statements by non-Europeans in the New World. If silence, as Greenblatt asserts at one point, is a form of wonderment or marveling at what is countenanced, and such a state is a precondition of appropriation, then the indigenous people of the New World certainly should have exercised greater possession over the invading Europeans. Their silence is, after all, preponderantly greater than the Europeans'.

But marveling, Greenblatt claims, can also be a form of accommodation of and to the strange, especially, he might have added, when one is left little choice in the matter, as was the case with the people of the New World. Marveling also has its improvisational virtues, and the pragmatic ends of the European explorers certainly found the means and protocols, linguistic and tactical, necessary for their fulfillment. While wonderment and marveling may have more benign faces, history certainly has little to show by way of such sanguine consequences in the encounter of cultures. And at one point Greenblatt himself betrays the less-than-benign repercussions of wonder as he contemplates the Dome of the Rock and the possibility of undertaking a critique of Zionist imperialism, which ideologically underwrites what he calls "Christian imperialism" in the New World (he overlooks the fact that he is citing the Old Testament in pointing out what the European Christians themselves often cited as justification for conquest). But, he confesses, he cannot bring himself as a Jew to formulate that critique. He suggests it only to admit to his ambivalences. His honesty is admirable, but it does prove corrosive to the more sanguine claims he would suggest for states of wonder and marveling, illustrating in the process how wonder and marvel could indeed mitigate insight and ethical impulse.

As different as Mason's and Greenblatt's books are, each, of course, is engendered by the calendar, I suspect Mason's less so as evidenced by its earnestness and intellectual excitement. Greenblatt's book consists of occasional pieces, sundry lectures pragmatically tied expressly to the historical moment.

DJELAL KADIR  
University of Oklahoma



SANDER GILMAN. *The Jew's Body*. New York: Routledge. 1991. Pp. ix, 303. Cloth \$45.00, paper \$15.95.

In the nineteenth-century popular and medical imagination, Jews were commonly said to suffer from a veritable menagerie of distinctive "diseases." Jews were cast as syphilitics, criminals, and psychotics; Jews were equated with prostitutes, Negroes, and sexual deviants. Jack the Ripper was imagined as a "foreign, syphilitic, mutilated butcher-Jew" of Eastern European extraction; he was even likened to the *shochet*, or ritual butcher, at a time when antivivisectionists were publicly denouncing the ritual slaughter of kosher meat. Sander Gilman's purpose in highlighting representations of Jewish bodily difference is not to draw a line between "real" and "fabled" (his quotes) aspects of the Jewish body, but rather to excavate and display some of the endless variations on the metaphor of "Jew as disease." His discussion centers on *fin-de-siècle* Europe but ranges broadly from the Biblical roots of Jewish language stereotypes, to Proust's discussion of "ethnic eczema," to assimilated Jewish ambivalence over Jackie Mason's authenticity.

Some of this is familiar, as when we hear about Francis Galton's construction of a composite image of the "essential" Jew from photographs of ten Jewish men and his facile claim to be able to discern "cold calculation" in the eyes of young Jewish schoolboys. Gilman moves refreshingly beyond outsider stereotyping, however, to explore how Jews responded to and reproduced such traditions. The social scientist Joseph Jacobs, for example, fancied himself able to identify Jewish eyes (more readily even than their notorious "nostrility"), even if all other facial features were obscured. Jacobs, though, attributed the "calculating" visage of Galton's composites not to any inherent constitutional impairments but rather to the hard battle of life endured by England's Jewish youth.

One of the most suggestive chapters is on what might be called the political pathology of the Jewish foot. The Jewish foot was the hidden sign of the Jewish body, the counterpart to the cloven-hoofed devil of the Middle Ages. In the nineteenth century, the Jew's flat feet were said to bar him (and the target was almost always a male) from military service and therefore from the rights of citizenship. Gilman again shows that this underlying premise of difference persisted even among many Jewish authors: Gustav Muskat, the Berlin orthopedist, conceded Jewish bodily deformity but attributed it to improper training; Elias Auerbach advocated a regimen of sport to reshape the Jewish body, following the Zionist Max Nordau's call for a "new Muscle Jew." Jewish flat feet were eventually added to the list of other distinctively Jewish maladies, alongside diabetes, hemorrhoids, tuberculosis, syphilis, and various "nervous encumbrances" said to plague the Jewish race (lunacy, hysteria, neurasthenia, and so on). Gilman also provocatively treats the history of the "nose job," an

operation popularized at the turn of the century by Jacques Joseph, the German-Jewish surgeon with dueling scars who sought to cure his (mainly Jewish) patients from depression by giving their Jewish noses "Gentile contours." Gilman provides an interesting contrast between Joseph's own scars-for-assimilation and his promotion of assimilation-through-surgery.

Some of Gilman's passing remarks are suggestive although unsupported, as when he states that Jews and prostitutes were linked via the metaphor of wasteful and illicit "spending" of semen through masturbation. The author's elaborate reinterpretation of Freud's misreading of the Dora case is a bit stretched, especially when he concludes that "the act of the female sucking on the penis of the male, a 'pathological' act as it represents the spread of disease (hysteria) to the daughter, is a sublimation of the act of the male sucking upon the penis of the male and spreading another disease, syphilis" (pp. 94–95). One need not be a radical empiricist to wonder how one would ever know whether to believe such a claim, or how one would convince a non-Freudian that the image of the Jewish nose "is a 'delicate' anti-Semitic reference to the phallus" (p. 126).

One curious omission from this wide-ranging volume is a discussion of the female Jewish body. Gilman readily concedes that he is concerned almost exclusively with images of the male Jewish body, although attention to readings and misreadings of the female body would have supplemented his account. Something might also have been said about whether and to what end conceptions of the bodily "sameness" of Jews have been advanced to counter the images of inherent "difference." There are cases, after all, where the negligence of difference has been as hurtful as its celebration. Gilman will no doubt have more to say on such matters.

ROBERT N. PROCTOR  
Pennsylvania State University

ELAZAR BARKAN. *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars*. New York: Cambridge University Press. 1992. Pp. xiv, 381. \$49.50.

Elazar Barkan's nuanced treatment of American and British anthropologists and biologists during the period between the world wars is an authoritative and significant contribution to the history of racial thought of the two major English-speaking countries. His study, based on a rich combination of archival, manuscript, and published sources as well as key secondary works, is a splendid example of the type of work that should be done in this subfield of intellectual history.

Barkan, I believe, succeeds in discrediting the myth—created by such historians and historically minded social scientists as Leon Poliakov, Thomas F. Gossett, Nancy Stepan, Michael Banton, and Hamil-

ton Cravens—which states that the response to Nazi racism led to the renunciation of theories of racial superiority on the part of scholars in the human sciences as early as the 1930s. Barkan demonstrates convincingly that the response to Nazi racism “was neither immediate nor of sufficient strength to discredit theories of racial superiority” and that, furthermore, as late as 1938 “only a small segment of the educated public had formulated its attitude on the question of race in response to the Nazi menace” (p. 1).

Nevertheless, Barkan concurs in the argument that Nazism had a decisive impact on British and American thinkers’ attitudes toward race. Barkan believes that the “uniqueness of Nazism underscored the immanent wickedness of racism, crystallizing the dichotomy between respectable bigotry within the middle class circles and evilness of the enemy” (p. 345). In so doing, one can account for what happened after World War II, when UNESCO published its statement in 1950 asserting that the race concept was a “social myth” and when the revolution spread to influence the *Brown v. Board of Education* decision and the Civil Rights movement. Barkan also attempts to demonstrate that there were sufficient preconditions for the triumph of an antiracist position in the human sciences even if Nazism had not emerged. He points out that the use of prominent antiracists such as Franz Boas, Otto Klineberg, Melville J. Herskovits, Ruth Benedict, Margaret Mead, and Leslie C. Dunn in the United States, and Lancelot Hogben, Lionel Penrose, and Julian Huxley in Great Britain, had succeeded before 1933 in countering the exaggerated claims of racists. Nevertheless, Barkan does not attribute to scientism a major motivating force on attitudes toward race. Rather, he declares unequivocally that “political beliefs” were more salient than “scientific commitments” (p. 343).

Despite the yeomanlike service that Barkan has performed, I have two reservations about his work. First, his uncritical approach to Boas’s writings on African Americans is surprising. His statement that “Boas was no racist, but he did reflect the values of his society” (p. 81) makes no sense. Further investigation would have revealed the tension in Boas’s writing between his lifelong belief in inherent racial differences and his commitment to cultural explanations of human behavior. Indeed, like most students of the human sciences of his period, Boas was also a prisoner of his times. Second, the book is poorly written and edited. It is repetitious and filled with such errors as “The present allegations that Keith have masterminded the Piltdown fraud would turn him into a Dickensian villain: a racist and a con” (pp. 52–53). Despite these reservations, here in its own terms is an instructive contribution to the growing body of literature on racial thought.

VERNON J. WILLIAMS, JR.  
Purdue University

EDWARD SHORTER. *From Paralysis to Fatigue: A History of Psychosomatic Illness in the Modern Era*. New York: Free Press. 1992. Pp. xii, 419. \$24.95.

Edward Shorter has written a series of provocative, readable, and intelligent books over the past decade on the social history of medicine, most recently a study of *Bedside Manners* (1985). He has now followed this work with a study of “psychosomatic illness in the modern era” that is no less provocative and readable. This study examines the relationship of mind and body as well as doctor and patient in the construction of diseases of the body. Shorter’s thesis is straightforward. He describes a core of related symptoms that cluster about the images of the hysteric and the neurasthenic in the late nineteenth century. They include physical symptoms such as impairment of movement and gait, lethargy, and difficulties in speaking and articulation, among others. He then asks what the relationship of these symptoms is to specific understandings of how the mind and the body interact. Shorter illustrates well that psychosomatic illnesses exist, that they and their symptom clusters are structured by the culture in which they are generated, and that the very discipline of medicine plays a role in structuring the psychosomatic response of patients seeking a physical definition for their psychological illnesses.

In illustrating the history of psychosomatic illnesses, Shorter risks a more complex set of associations, not all of which are germane to his central thesis and some of which lead him to make claims that are more difficult to support. Thus, he begins his study with a series of retrospective diagnoses looking at the diagnostic criteria of specific forms of illness (such as hypochondria) in the early modern era. He sees these clusters of symptoms as the natural precursors of the late-nineteenth-century diseases that he argues (quite correctly) are at the source of debates about the nature of psychosomatic illness. This type of retrospective diagnosis is always risky. Here perhaps less so. But one is ill at ease about claims that symptom clusters in the early eighteenth century have the same significance for patients and doctors as they do in the late nineteenth century. Indeed, the entire discourse about “illnesses of the will,” a topic about which John Smith has so ably written in an essay evidently unknown to Shorter (*Genders* 6 [1989], 102–24), became possible only in the nineteenth century, when certain patterns of representing internal mental states had been established in European culture. Simply assuming a continuity to the late nineteenth century does not aid the power of his argument.

In this volume, Shorter also has a bit of a quibble with “science.” While he does not want to see science as producing a series of truths, he periodically falls into the trap that assumes this. Thus, he describes Paul Dubois as a psychotherapist and contrasts him with Jules-Joseph Dejerine, who “was much more of a

scientist" (p. 247). This distinction is problematic, as both men clearly saw themselves as both clinicians and researchers. The contrast between "therapy" and "science" is Shorter's; it does not belong to the world of nineteenth-century French medicine.

Shorter also introduces a topic of special interest to me as a subtext throughout his narrative: the special role of Jews in the culture of medicine (as physicians and patients) in nineteenth-century Europe. While recognizing this as an important topic, Shorter is often at a loss about what to do with it. This leads to rather enigmatic turns, such as his acknowledgement that all of the German physicians who worked on the problem of spinal irritation were Jews, but he offers no sense of what this does or does not mean (p. 35). Shorter's difficulty lies in his inability to understand that being Jewish was a racial rather than a religious label in the terms of the science of the age (p. 133). The claim about the reality of the charges about Jewish predisposition to disease, which Shorter evoked elsewhere in a noteworthy essay on this topic (*Medical History* 33 [1989], 149–83), is here suspended, and the problems associated with the comparative stigma of being Jewish and being mentally ill are bracketed out of the discussion. One does hope that Shorter returns to this topic, as he has promised to do.

All in all, however, Shorter's book is deeply provocative, and nowhere more so than in his explanations of contemporary Western discussions of psychosomatic illness. His reading of chronic fatigue syndrome as the neurasthenia of the late twentieth century in its symptomology, its popularization, and its rejection of any psychosomatic etiology is a powerful claim. The proposed reintroduction of neurasthenia into DSM-IV evokes the relationship between chronic fatigue syndrome and neurasthenia. (I would have cared in this case, however, for some greater sophistication, such as an admission that there may well be a physical etiology in some cases lumped under the category of chronic fatigue syndrome, given the ability of virologists to present a profile of a physical illness in particular instances.)

Shorter's study presents a powerful case for why we today wish to avoid seeing illness as psychosomatic and what pressures on contemporary Western culture make the "physical" into the only sphere of illness that is "real." (And this in spite of Freud and the impact of psychoanalysis on our contemporary understanding of the relationship between the body and the mind.) If for this reason alone, Shorter's powerful study is of great value and will be of true interest to all social and cultural historians who deal with the role of illness in culture and culture in illness.

SANDER L. GILMAN  
Cornell University

EDWARD E. GORDON. *Centuries of Tutoring: A History of Alternative Education in America and Western Europe*.

Assisted by ELAINE H. GORDON. Lanham, Md.: University Press of America. 1990. Pp. 437.

"The history of tutoring," concludes Edward E. Gordon, "is largely the study of individuals, not institutions" (p. 330). This statement well capsulizes one major methodological problem for a survey of tutoring between 1500 B.C. and the present: how to move from individual example to broad social trends. In reality, Gordon's first discussion of individual tutors—Socrates, Plato, and Aristotle—begins after 500 (not 1500) B.C., and the ancient and medieval periods are dispensed with in less than thirty pages. As the author acknowledges, his study looks most closely at tutoring between the Renaissance and 1900 and relies mostly on examples from Anglo-American sources. The result is a work whose strength is the presentation of interesting instances of tutor-pupil relationships and tutors' thoughts on the nature of education. Its weaknesses are methodological and historiographical.

Gordon's two major and worthy goals are to link the history of tutoring to the history of childhood and to highlight the "significant contribution to modern educational philosophy" made by men and women "philosopher-tutors" (p. 1) before the development of comprehensive public schooling in the late nineteenth and twentieth centuries. Although Gordon has appropriately identified books by Philippe Ariès and Linda Pollock as basic sources for the history of childhood, he provides no systematic discussion of the major historiographical issues posed by historians of childhood during the three decades since the publication of Ariès's pathbreaking work.

For the nineteenth century, discussion of the development of public schooling on the European side of the Atlantic is most inadequate. Gordon not only ignores much recent work on French public schooling and parental demand for schools before the laws of 1881–82 made primary education compulsory and, in public schools, free, but also, for his more central case of England, the importance of the educational reform act of 1870. Thus, the uninformed reader is left with the idea that primary schools in England were far more unavailable before 1914 than was the case (p. 217). Consultation of a newer survey such as Mary Jo Maynes's *Schooling in Western Europe: A Social History* (1985) would have helped correct the impression that there was little educational opportunity for the popular classes who could not benefit from the tutors hired by middle-class and upper-class families. In turn, Gordon's frequent reliance on Will Durant as a source is likely to disconcert many professional historians. The chapter heading designating the seventeenth century rather than the eighteenth as "the age of reason" is also a surprise.

A final problem stems from an inadequate effort to clarify, with statistics, the importance of tutoring for upper and middle-class families. Gordon does address the English case by using Nicholas Hans's older

study based on entries in the *British Dictionary of National Biography*, but he attempts nothing comparable with the use of American census data. Because of Gordon's commendable effort to treat the tutoring of girls as well as boys, we learn that the English census of 1861 recorded 24,000 governesses working in private households. More often, however, the reader must rely on seemingly random anecdotes to assess the importance of tutoring for a particular middle or upper-class generation.

For historians of education, childhood, and the family, one of Gordon's major contributions is his documentation. He offers a ten-page appendix of books detailing the "tutorial ideal" since 400 B.C., a list of more than thirty words used to designate tutors in various European languages, a thirteen-page appendix listing works of English fiction that depicted governesses between 1749 and 1958, and a twenty-five-page list of primary sources, drawn mostly from published works but also from some American manuscript collections. Historians of education often observe that there is far more to the history of learning than the history of schooling, and it is to Gordon's credit that he has attempted to document this important point.

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PETER BROCK. *Freedom from Violence: Sectarian Nonresistance from the Middle Ages to the Great War*. Buffalo, N.Y.: University of Toronto Press. 1991. Pp. x, 385. \$55.00.

PETER BROCK. *Freedom from War: Nonsectarian Pacifism, 1814-1914*. Buffalo, N.Y.: University of Toronto Press. 1991. Pp. ix, 436. \$60.00.

These two recent books by Peter Brock on the history of nonviolence and pacifism are useful additions to the literature of the field. Nonetheless, scholars familiar with Brock's important earlier works in this area will find these new volumes disappointing and perplexing. Brock presents here two surveys of pacifist history. The first, *Freedom from Violence*, focuses on "sectarian nonresistance" from medieval times to 1914, while the second, *Freedom from War*, focuses on "nonsectarian pacifism" in the century from 1814 to 1914.

What is disappointing is the fact that large portions of the two volumes retrace, in abridged form, ground covered more extensively in Brock's earlier works (especially *Pacifism in the United States: From the Colonial Era to the First World War* [1968] and *Pacifism in Europe to 1914* [1972]).

This is also perplexing, because Brock fails to offer any explanation of the relationship between these volumes and the earlier works. In general, *Freedom from Violence* and *Freedom from War* appear to be directed to a more general audience than were *Pacifism in the United States* and *Pacifism in Europe*, but

Brock does not say so. Somewhat to the contrary, in the preface to *Freedom from Violence*, Brock tells us that he has provided "a full scholarly apparatus" in his footnotes (p. ix). Actually, much of the detail and scholarly references of the earlier works is omitted, but the new volumes do provide many new references to recent materials.

Most confusing and frustrating, from a scholarly viewpoint, is what appears to be deliberate omission throughout these two volumes of citations directing the reader to the fuller treatment and bibliographic references provided by Brock on many topics in those earlier works. For example, consider Brock's remark in *Freedom from Violence*: "The Hutterites constitute the best example of *what I have designated elsewhere* as separational pacifism" (p. 61, emphasis added). Brock does not tell the reader the location of this "elsewhere," and he uses this vague form of reference in the same way in several other places. In this case, "elsewhere" is *Pacifism in Europe to 1914*, where Brock had discussed "separational pacifism" (pp. 474-75) in the context of a general typology. In many cases, the wording in these two volumes is almost identical with that in the earlier works (for example, on the communism of the Hutterites, compare *Freedom from Violence*, p. 50, to *Pacifism in Europe*, p. 235). One must assume that Princeton University Press, which held the copyright on the earlier volumes, has given its permission for such borrowings, although nowhere in these volumes is this acknowledged. Brock does explicitly cite his own works in other cases (for example, "The above sentences have been drawn largely from my essay 'The Polish Identity,'" *Freedom from Violence*, p. 314, n. 1). This leaves us all the more puzzled at the absence of any mention of the far more extensive overlap with *Pacifism in the United States* and *Pacifism in Europe*. Whatever one may think of this in terms of scholarly ethics, the practical problem is that Brock leaves entirely to the reader the tedious work of page-by-page and note-by-note comparisons to determine, whether for research or teaching purposes, which topics were never covered in the earlier works, which were covered and more thoroughly treated, and which have been accorded some new analysis.

In general, the major changes are of three kinds: materials dropped or added, thematic reorganization, and a more didactic, judgmental tone. The material covered in the earlier volumes has been greatly compressed, some portions have been dropped entirely, and some new material, new source references, and new chapters have been added. Chapters of both earlier books dealing with Quaker pacifism have been omitted, presumably covered in Brock's volume *The Quaker Peace Testimony 1660 to 1914* (1990). Unaccountably, Brock offers no explanation for this major omission in *Freedom from Violence* and alludes to it only tangentially in the preface to *Freedom from War*, where he mentions having dealt with the Quakers and other pacifist sects "elsewhere." *The Quaker Peace Testimony* is mentioned on the book



jackets and in several footnotes, but omitted from the bibliographies of both 1991 volumes.

New chapters appear in both volumes. In *Freedom from Violence*, there are two, one on the Plymouth Brethren (which Brock notes is drawn from an article of his published in *Church History* in 1984) and one on "Christian Pacifism in Denmark and Sweden to 1914." The latter is included without explanation as an appendix rather than a chapter, presumably because it did not fit the model of "sectarian nonresistance." *Freedom from War* offers a larger number of new chapters (about one-third of the volume), on pacifism in Victorian Wales, Henry Richard, Tolstoyan war resistance in Bulgaria and the Netherlands, a Slovak case study, nonviolent anarchism in Austria-Hungary, Gandhi in South Africa, and Christian pacifism in Japan.

The material in both volumes has been reorganized, dropping the geographic boundaries of the earlier works and directing attention instead to a categorical division between "sectarian" and "nonsectarian" pacifism, or more specifically, "sectarian nonresistance" and "nonsectarian pacifism."

Brock does not offer an explicit definition of "sectarian nonresistance," but he describes the nonresistant sects (such as the Brethren, Mennonites, Hutterites, and others) as renouncing war and violence within their own communities and by their own members. But Brock argues that these sects have held a pessimistic view of human society at large that led them to discount the possibility of universal peace on earth and to accept "the magistrate's sword" as inescapable in the outside "pagan" world. Sectarian nonresisters, in Brock's view, represent a form of pacifism that he calls "separational," as opposed to the "integrational" forms of pacifism described in *Freedom from War*. Brock has also ventured here to offer more judgmental remarks, especially negative toward sectarian or "separational" pacifism, that were absent from (or at most implicit in) his earlier volumes.

Under the term "nonsectarian" Brock focuses primarily on Christian-based pacifism not requiring membership in a particular Christian sect. Secular, socialist, or humanist pacifism as well as pacifism based in other religious traditions receive little attention and appear to be treated as either offshoots of Christianity or not conforming to Brock's definition of pacifism, which he separates from internationalism and "pacifism" (*Freedom from War*, p. vii).

Many readers will be disappointed to find that Brock has not transcended the limitations of his earlier works with respect to the ideas and contributions of groups of non-European or non-Christian descent and culture, and of women of any culture. The treatment of all of these is primarily by omission, cursory mention, and condescension. Only one woman, the Seventh-day Adventist Ellen White, is given significant attention in *Freedom from Violence*; no woman is accorded even a full paragraph of discussion in the text of *Freedom from War*. Jane Addams

and some others are given a few lines; Bertha von Suttner is relegated to a footnote.

A paragraph devoted mainly to the black abolitionist William Whipper is introduced by the statement: "The contribution to nonresistance of blacks as a group was considerably less than that of women—understandably so in view of their numbers and position in northern society at that time" (*Freedom from Violence*, p. 80). While the notice given to Whipper and the passing mention of Sojourner Truth and Frederick Douglass here is better than their total absence in Brock's earlier work, *Pacifism in the United States*, the dismissive and patronizing framing, infused with solipsism, brings us only one step forward, two steps back.

The main line of argument of the two volumes appears to be that "before 1814 pacifism was almost exclusively sectarian," and specifically a phenomenon of Christian Protestant sectarianism, while "henceforward the pacifist impulse . . . expands and finds increasing numbers of adherents, at first in other Protestant denominations and eventually among non-Protestants and even persons of other faiths than the Christian or of no religion at all" (*Freedom from War*, pp. vii–viii). Brock explicitly states that pacifism (at least what he calls "pacifism *sensu stricto*" or "absolute" pacifism), is essentially Christian in origin and nature: "Historic pacifism, including the nonsectarian pacifism of the nineteenth century, emerged from a religious impulse, for its antimilitarism derived from the Christian faith of its adherents" (*Freedom from War*, p. 307).

Brock presents this as true even of non-Christian pacifists such as Hindus, Buddhists, and Jews, representing their pacifism as though it were an outgrowth of European Christian pacifism: "Indeed, from at least the end of the Second World War . . . the pacifist community was beginning to expand . . . reaching out beyond Europe and North America into other parts of the globe . . . [and] also expanding vertically so as to include in its sphere of influence not only secular humanists as well as Roman Catholics and members of other Christian denominations hitherto generally unresponsive to the pacifist message, but also persons of other religious faiths. At last, Jews and Buddhists (not to mention Baha'is or some North American Indians)—and even a few Muslims—began to uncover the nonviolent components that had existed in their past and to develop them now in the struggle for a peaceful human future" (*Freedom from War*, p. 307, emphasis added).

Brock however, offers in this volume only one chapter on Gandhi and one on "The Dawn of Christian Pacifism in Japan" to illustrate the global scope of this vision of expanding pacifism, with hardly a word about Jewish or Muslim pacifism, let alone any more than this passing mention of the peace traditions of Native Americans. The few pages devoted to the influence of Jainism, Hinduism, and Buddhism on Gandhi and in Japan are mainly devoted to showing



that these religions were not fully "pacifist" in the sense preferred by Brock, but this has not prevented him from devoting hundreds of pages to Christian "pacifists," Christian sects, and other Europeans whom he shows were neither absolute nor consistent in their adherence to "pacifism *sensu stricto*." The double standard of Eurocentrism and Christian-centrism, and the imperial language of the passage quoted above, underlie the whole approach of these books and constitute a fundamental failing of both.

In short, these two books will have some uses for scholars and teachers interested mainly in a Christian-centered account of the history of pacifism, but researchers may find their work hampered by Brock's curious references (or omission of references) to "elsewhere," and readers interested in a broad-visioned history of movements for freedom from violence and war will not find it here.

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WILLIAM O. WALKER III. *Opium and Foreign Policy: The Anglo-American Search for Order in Asia, 1912-1954*. Chapel Hill: University of North Carolina Press. 1991. Pp. xvi, 345. \$39.95.

In a previous book dealing with the Americas (*Drug Control in the Americas* [1981, 1989]) and several provocative articles, William O. Walker III has studied the convergence of the foreign policy of drug control, culture, and national security. In this book, Walker applies his paradigm to Asia, primarily China, in the context of the Anglo-American search for order from 1912 to 1954. After establishing the background for the opium problem in China and international attempts to control it, Walker discusses the budding signs of reform following the Revolution of 1911, supported by Yuan Shih-k'ai and Chinese revolutionary nationalism. He then examines how these hopes were dashed over the next two decades by financially pressed warlords and the Kuomintang government who depended on revenues squeezed from the opium traffic; he also faults the major foreign powers who were suspicious of one another and had different agendas. Walker then reinterprets the march toward World War II as he analyzes the problems and dilemmas of drug control within the context of Japanese expansion against the weak Kuomintang regime that increasingly looked to the United States for salvation. The United States thereafter was forced to confront drug control as a question of national security and not merely morality. Although the war assured the ascendancy of the United States in East Asia, the drug problem complicated Washington's simultaneous efforts to defeat Japan, manage the difficult Chiang Kai-shek, and persuade Britain and France to dissolve their colonial empires. Finally, Walker tackles the first postwar decade, when American policy makers frantically tried and failed to

achieve their objectives of supporting Asian nationalism and anticolonialism, containing communism, and eliminating drug trafficking.

Walker's thesis has three primary elements. First, the American effort to eliminate drug use and trafficking, derived from ignorance and ethnocentric moral imperatives, was doomed from the start; and it remains futile because policy makers have failed to understand the economic and the cultural bases for narcotics in Asian societies. This weakness was magnified by the addition of the law-and-order perspective, best represented by Harry Anslinger and the Federal Bureau of Narcotics. Second, beginning in the 1930s, the goal of total suppression was subordinated to and actually subverted by America's perceived security needs. Just as Washington blamed Tokyo for the drug problem in the 1930s, so American policy makers singled out Communist China in the 1950s. In the latter instance, American behavior was particularly outrageous, for while Japan was certainly tainted, the People's Republic had essentially eradicated drug use and trafficking and American agents abetted drug running by anticommunist clients. This only underscored America's failure to define a coherent anticommunist strategy in Southeast Asia. Finally, Walker argues that competing interests and perceptions regarding drug control significantly interfered with attempts to establish an effective Anglo-American order in East Asia. For London, the bankruptcy of Washington's policy by 1955 made joint efforts virtually impossible.

Walker makes a compelling case for the place of drug control in Asian diplomacy, although he sometimes implies that it had a greater impact at the highest level of policy making than was actually true. Although certainly it is useful at times, Walker tends to belabor the concept of "order." He also fails to define more precisely just what constituted the American and British Asian "orders" at several junctures. Whitehall may have had a clearer conception of this goal, yet it is doubtful if Washington had definitely formulated its East Asian "order" in the late 1930s. Walker's discussion of the Anglo-American relationship is sometimes confusing. Thus, while the United States may have adopted a more cooperative public stance toward the Opium Advisory Committee in the late 1920s, it was more committed than ever to the total suppression of opium. So was there real agreement on policy?

These comments aside, Walker has produced a significant, valuable, and thoughtful study. His research in the primary sources is exhaustive. His critique is balanced and his judgments for the postwar era, where there are gaps in documentation, are cautious. This work further places Walker at the forefront of scholars seeking to invigorate diplomatic history by integrating the cultural dimension with the sound use of traditional sources.

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## ANCIENT

SARAH B. POMEROY, editor. *Women's History and Ancient History*. Chapel Hill: University of North Carolina Press. 1991. Pp. xv, 317. Cloth \$39.95, paper \$13.95.

All scholars working on women and the family in the Greek and Roman world are indebted to Sarah B. Pomeroy for her own work and for the panels, workshops, and bibliographies she has initiated. The focus of the present volume is on "the relationship between public and private in the lives of women in antiquity" (p. xi). Seven of the twelve articles published here originated from the Berkshire Conference or a National Endowment for the Humanities Summer Seminar in 1987. The book, intended to "illuminate aspects of the full range of ancient history from archaic Greece to late antiquity" (p. xiii), consists of seven articles on the Greek world, four on the Roman, and one on Jews and Christians. Some are paired: Jane McIntosh Snyder and Marilyn B. Skinner on the women authors Sappho and Nossis, Ann Ellis Hanson and Lesley Dean-Jones on Greek medical theory about women's bodies. Cynthia B. Patterson contributes an account of Athenian marriage, Claude Mossé of women's politico-economic role in the Spartan revolutions of the third century B.C., Elizabeth Carney of the emergence of titles for Hellenistic royal women. For Rome, Mireille Corbier writes on the family strategies of the aristocracy, Diana Delia argues against recent views of Fulvia, Natalie Boymel Kampen examines women at the intersection between private and public in relief sculpture, and Mary Taliaferro Boatwright shows Plancia Magna to be an exemplar of a benefactress. Finally, Shaye J. D. Cohen traces taboos on menstruation in Judaism and Christianity down to our own day. There is much here of interest.

The similarity of approach (historical, where collections of essays on women's studies have often been predominantly literary; feminist; and rooted in sources) and overlap of themes (for example, elites, marriage) produce a certain coherence. But despite the collaborative approach, the themes tackled here with serious research and in a spirit of lively inquiry do not add up to a comprehensive study of women in ancient, or even Mediterranean, society. Perhaps the editor's just claim that all kinds of history (political and economic as well as social) must fully incorporate women undercuts such collections. Perhaps the evolution of work since 1987 makes her remark "that more pages are devoted to Greek than to Roman women is a reflection of the state of contemporary scholarship on women in antiquity" (p. xiii) look old-fashioned. Evolution has been rapid and scholarly interchange, while not neglecting comparisons between Athens or Alexandria and Rome, now focuses on more sharply defined themes, periods, and geographical areas. Already in 1986 we had had a set of conference papers edited by Beryl Rawson (*The*

*Family in Ancient Rome: New Perspectives*), which pioneered routes for further work in that field (partly represented in the proceedings of the second Canberra conference, *Marriage, Divorce and Children in Ancient Rome* [1991]). The results of a Paris conference on the Roman family have also been published (*Parenté et stratégies familiales dans l'antiquité romaine* [1990]). Other attractive trends made possible by the increasing sophistication of scholarship on Greco-Roman women and the family are the encouragement of exchange of ideas across national and linguistic boundaries (represented by the inclusion of Mossé and Corbier in this volume) and attempts to make diachronic comparisons (exemplified by David I. Kertzer and Richard P. Saller, eds., *The Family in Italy from Antiquity to the Present* [1991]). Inevitably in an active discipline what was at the forefront of our thinking in 1987 looks a little dated in 1993: what is published marks a new starting point, not a definitive resting place.

Reviewers of collections are often more competent to comment on some essays than others. As a Roman historian, I shall happily direct students to these accessible reviews of Greek poets (while regretting the nonappearance of Sulpicia) or the magisterial surveys of classic gynecology (while wishing for an essay dedicated to Galen and Soranus as well). Patterson's wide-ranging and judicious study on Athenian customs will make a useful *mise-au-point* for students of Roman marriage. Contrasts and comparisons about the political effectiveness and recognition of royal women emerge from Mossé and Carney. Cohen fills a surprising gap in Judeo-Christian scholarship: the nonspecialist is grateful to be able to adduce his well-documented and persuasive account of attitudes to menstruants as comparative material. Corbier's article (revised and rather too literally translated from her paper in *Annales* 42 [1987]) parallels "Divorce and Adoption as Roman Family Strategies" in Rawson and "Constructing Kinship in Rome" in Kertzer and Saller. Here she examines how Romans sought to secure the survival of the lineage, make alliances between families, and dispose of property. Marked by its lucidity, wide-ranging illustrations, and grasp of other comparable societies, the article will make a useful introduction for anglophone undergraduates. She emphasizes the presence of contradictory objectives and ideals in Rome but finds the chief distinguishing features in the rights of women over property and the two vital "cards," which aristocrats held and used in maximizing the success of their families and which the church later took away: divorce and adoption. Delia offers a careful discussion of the sources in which she is acute on the difference between perceptions and reality; she is alert to the dangers of mistaking invective for fact and of extrapolating tendentious data on Fulvia's third marriage to the period of her first two. I am inclined, however, to think that there was rather more fire than she allows behind the smoke fanned by Cicero and Octavian.

Other Roman women before and after meddled in public affairs: the losers were attacked in the literary tradition as over-sexed or masculine; those on an author's side were model wives and mothers who intervened on grounds of clemency or patriotism. Kampen provides a valuable discussion of women on historical reliefs, focusing on the scene showing Septimius Severus and Julia Domna and family at a sacrifice on the arch at Lepcis Magna. She shows that women are rare on these large public monuments (contrast this with the frequent appearance of women of the imperial family on coins) and that they belong chiefly to two categories that act as parallel symbols: women of the people or of defeated enemies, both representing an entire community, and women of the imperial family (sometimes in association with Vestals, state officials) representing the blending of family and state. Domna watches her husband and sons clasp hands and "becomes the sign that this act is both familial and dynastic, personal and political" (p. 231). Finally, Boatwright in a highly competent article discusses the extraordinary euergetism of Plancia Magna of Perge in Pamphylia, who was honored as eponymous magistrate, priestess, and "daughter of the city," and who early in the reign of Hadrian paid for the grand southern gate, with statues of gods and heroes, her father and brother, and male and female members of the imperial family. The dialogue between Kampen and Boatwright forms a point of departure for new research.

The volume will be useful for some university courses. A second edition would be improved by the addition of a general bibliography and the augmentation of the index.

SUSAN TREGGIARI  
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SUSAN TREGGIARI. *Roman Marriage: Iusti Coniuges from the Time of Cicero to the Time of Ulpian*. New York: Clarendon Press of Oxford University Press. 1991. Pp. xv, 578. \$120.00.

Susan Treggiari assembles and analyzes a vast array of literary and legal sources in a magisterial examination of the institution of Roman marriage in the period ranging from the early first century B.C. to the beginning of the third century A.D. The result is a study of fundamental importance that is required reading for anyone who teaches or writes on Roman society and for historians of the family in other ages as well. Even compared to the high quality of much recent work on the Roman family in classical times, this book stands out as a superb culmination of recent advances in knowledge and an obvious point of departure for future work on this theme.

Treggiari arranges her book along the lines of what can best be described as the "life-cycle" of the Roman family. Beginning with definitions and legal requirements for marriage, Treggiari surveys criteria for

spouse selection, the politics of arranging a betrothal, the customary practices associated with weddings, affectionate marriage as an ideal, sexual conduct, property relations, living conditions, and separation. This last section treats divorce and death, and closes with brief remarks on remarriage, where "the cycle began again" (p. 502).

Six appendixes deal with alleged adulterers, prosecutions for adultery, remarks of philosophers on sexual matters, magistrates from the decade 60–50 B.C. whose wives can be identified, and attested divorces in the late Republic and *separatim* early Principate. All are in essence selections from the broad pool of evidence Treggiari has mastered, and they offer a convenience that signals the work's double orientation as a tool of teaching and research. The value is further enhanced by a chronological chart, a stemma of the Julio-Claudian family, and a set of fine indexes (on texts, persons, and subjects). All principal texts are translated into precise and idiomatic English.

Not surprisingly, Treggiari builds on the considerable body of her own work on Roman domestic life. This includes study of "irregular" unions such as respectable concubinage, which often functioned as a type of second marriage for elite males, and *contubernium*, a type of union that began when at least one of the partners was a slave. But the richness and complexity of Roman "quasi-marital" relationships almost pale beside what we learn of *matrimonium iustum* (full-status marriage). The wealth of detail alone makes the volume indispensable, but it is also true that the author has advanced the state of our knowledge on a number of individual topics. Two deserve special emphasis: life in the household and divorce.

Accounts of life in the Roman household have tended to focus on the activities of males, an imbalance Treggiari redresses in a chapter entitled "*Domus*." This richly detailed and fascinating portrayal of spousal relations and daily conduct begins with a survey of recent advances made by students of Roman demography, above all on mortality, age at marriage, and reproductivity. Treggiari then examines family structure, placing the nuclear core in the context of an extensive upper-class household and describing the impact of events such as divorce, death of spouse, and remarriage. The final section, devoted to married life, is perhaps the most interesting, including a wealth of information on the social role and activities of married women and relations between husbands and wives (where we find, for example, evidence for what behaviors constituted "mental cruelty" among the Romans).

At last we have a full and satisfactory account of Roman divorce (see also the author's excellent essay, "Divorce Roman Style: How Easy and How Frequent Was It?" in B. Rawson, ed., *Marriage, Divorce and Children in Ancient Rome* [1991], 31–46). As Treggiari shows, a firm corollary of the bilaterality of marriage was the unilateral nature of divorce. Divorce, like

marriage, was essentially a private affair, with great discretion allowed to the principals. Despite the relative ease of dissolving a marriage, the parties were not beyond criticism and divorce itself was typically thought a misfortune. Commentators whose own societies place severe restrictions on dissolution of marriages have tended to confuse freedom to divorce with frequency of divorce: in fact, Treggiari's provisional total of divorces attested during the early Principate (Augustus to Domitian) amounts to only twenty-seven examples (see appendix 6). We learn that, contrary to the beliefs of modern scholars who have preferred to moralize about "Roman decadence," the fact that spouses were not compelled to remain in loveless unions may well have strengthened the institution of Roman marriage.

The only part of the book that may provoke significant disagreement is the discussion on the double standard of sexual conduct for males and females, and particularly the argument that biology explains this (pp. 311–19). Treggiari traces the double standard to the primate ancestors of humans, speculating that the different and complementary attitudes of men and women toward sexual behavior derive from the contrasting experiences of each gender in "the period of ape-life in the trees" (p. 319). This thesis runs against the grain of the recent trend, inspired in great part by Michel Foucault, that views sexuality as by and large a social construct, and so redefines even some elements that were once regarded as purely biological facts, such as fertility and the human body.

Unlike some other recent studies of the Roman family, this book does not present the relationship between law and life as simple and straightforward. Nor does it describe the evolution of each one as a linear development, whereby changes in social values and practices are followed, often at a distance of decades, by corresponding adjustments in legal rules. As Treggiari shows, the law changes, but not always in one direction, and it arises from the balancing of ever-present interests more than as a reflection of steady-state historical trends. Allied to this observation is the constant recognition that when the Romans attempted to regulate especially delicate matters like sexual conduct, the results were often inadequate, self-contradictory, or just bad law.

It is easy to predict that this volume will become a standard work of reference for students of Roman marriage and related fields. Treggiari's empirical approach, fortified by vast learning and a superior sophistication of argument, has raised the subject to a new plane, making it harder than ever, for example, to prefer the peculiar though influential views of Paul Veyne on Roman family life. Of supreme importance is the conclusion that "instead of one simple pattern into which lives or marriages might theoretically fit, there were many" (p. 504). This hard-won recognition of the vigor and complexity of the institution of Roman marriage should give a fair idea of the vol-

ume's immense value for those who intend to work in this field. Here is a firm foundation on which to build.

THOMAS A. J. MCGINN  
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RONALD H. SACK. *Images of Nebuchadnezzar: The Emergence of a Legend*. Selinsgrove, N.J.: Susquehanna University Press or Associated University Presses, Toronto. 1991. Pp. 143. \$27.50.

Historiographic research has recently become an issue in Mesopotamian studies, where inquiry had been limited to verifying the historicity of a text rather than to examining the cultural attitudes of the writers (Mario Liverani, *Orientalia* 42 [1973], 178–94). Ronald H. Sack's new book (basically an expanded reworking for a more general audience of his article in *Mesopotamia* 17 [1982], 67–131) is a welcome addition to this new historiographic spirit. While not a biography or commentary on the Chaldean period, his topic is the career of the Chaldean king Nebuchadnezzar II (604–562 B.C.). Although there has been recent work on this subject (D. J. Wiseman, *Nebuchadnezzar and Babylon* [1985]), Sack does not duplicate material but rather surveys the neglected secondary classical and Hebrew sources about the monarch and compares the variety of cultural attitudes in them. He also attempts to grasp the nature of the preservation of the cuneiform tradition in other cultures. Sack utilizes research about folklore (drawing on B. Toelken, *The Dynamics of Folklore* [1975]), concluding that later sources about Nebuchadnezzar came not only from oral but also from written transmissions. For example, the Greek historian Megasthenes appeared to rely on Persian apocalyptic literature, while Berossus depended on cuneiform sources.

The classical writers concentrated on Nebuchadnezzar's building projects rather than his conquests, since they appealed to an audience charmed by Babylon's structures and their permanence; thus, they associated them with a god-like personage. Sack blames Herodotus for setting this folkloristic tone, confusing historical truth by combining characteristics of Nebuchadnezzar (who is not mentioned by name) with other individuals. He does not believe that Herodotus visited Babylon, which ignores evidence that he correctly described its domestic architecture (O. Ravn, *Herodotus' Description of Babylon* [1942], 79–80).

Because of Nebuchadnezzar's conquest of Judah, destruction of the Solomonic temple, and deportation policy, the Jewish sources viewed the Chaldean king in a different manner than the classical writers. The rabbinic sources attempted to keep his memory alive in oral and written folkloristic tradition and thus created negative images, some of which are reminiscent of the Chaldean king Nabonidus (556–539 B.C.). The writer of Daniel merged the two sources, accord-



ing to Sack, borrowing material associated with the later king, not because he was ignorant or confused, but because Nabonidus appeared in Persian sources that fit the situation involving Nebuchadnezzar. Thus, historians and literary critics have been confused by Daniel because of a misunderstanding of the author's intention. The Jews had a correct view of history but desired to fulfill their didactic purposes, which were realized by a portrayal that combined history and fiction.

There are, however, objections to Sack's view of the source material in Daniel about Nebuchadnezzar. Daniel's apocalyptic material appears to be a literary form in a Chaldean setting originating in the Persian rather than in the Hellenistic period (Mary Boyce, *A History of Zoroastrianism* [1975], 190–91), while predictive prophecy is found in Assyrian texts (A. K. Grayson and W. G. Lambert, *Journal of Cuneiform Studies* 18 [1964], 7–30; and W. Hallo, *Israel Exploration Journal* 16 [1966], 231–42). The four ages of Daniel may refer to the ages of Hesiod (Peter Walcot, *Hesiod and the Near East* [1966], 85–86) or Zoroaster (W. G. Lambert, *The Background of Jewish Apocalyptic* [1978], 8). Moreover, the similarities of the Prayer of Nabonidus and Daniel are superficial; Nabonidus's "madness" is based on a mistranslation of a broken text (Wiseman, p. 104). In sum, Daniel was not dependent on one tradition. Except for the objections raised, this work is sound in method and should provoke similar types of inquiry.

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#### MEDIEVAL

R. HOWARD BLOCH. *Medieval Misogyny and the Invention of Western Romantic Love*. Chicago: University of Chicago Press. 1991. Pp. ix, 298. Cloth \$45.00, paper \$17.95.

The reduction of women to objects of male thought has long preoccupied feminist scholars. In this context, R. Howard Bloch contends that a few *topoi*, or generally held assumptions, combined and recombined in medieval texts, reduce women to "an essence," thereby removing them from the realm of events. He situates "medieval misogyny" between two literary revolutions. The first is the construction of a "Christian gender model" by the early church fathers: a feminization of the flesh that is identified as the source of all evil. The ideology of virginity forms a bridge to the second revolution: the idealization of sexuality that proclaimed woman the source of all good. Thus, "negative and positive fetishizations of the feminine work to identical effect" and their seeming polarity is the product of material conditions (p. 11). I, for one, have no quarrel with this general conclusion.

The specifics of this well-intentioned book, how-

ever, are troubling. No one has ever systematically demonstrated a specifically Christian form of misogyny, and Bloch does not succeed in the enterprise either. The misogynist diatribes of the fathers rarely add anything to their pagan models. Moreover, Bloch fudges the whole question with his idea of interchangeable *topoi*. One example illustrates a host of difficulties. To draw Jerome's antimarital stance from Augustine's vision of the Fall of Man is not only anachronistic but ignores pagan tradition from Hesiod through Juvenal, which is freely quoted in the treatise against Jovinian. Jerome's genuinely original material appears in his description of the pains of marriage for women.

Bloch fails to see that there was another Christianity, woman's Christianity, that provoked misogynistic reactions. He blithely dismisses female agency as a cause of male misogyny, branding the connection as illogical (p. 76). Accepting the misconception that asceticism was a male invention, he rejects the notion that communities of assertive female virgins might have provoked men to try to discipline them. He maintains that the characterization of women as the devil's gateway and as brides of Christ are "essentializations" representing two apparently contradictory sides of the same issue. This global view entirely conceals Tertullian, who coined both phrases in a single treatise against women's claims to be ungendered by virginity. Thus, Bloch simply vanishes into an amorphous sea of texts and his subjects are transformed into meaningless *topoi*. A problem in understanding a man (and his emulators) is transformed into a problem in understanding Christianity.

Similarly, romantic love is seen as a continuation of the same impulse to "essentialization" softened by a need to respect the material power of twelfth-century women. There is a fundamental historical error here. The south of France generally lagged behind the north in developing institutions that deprived women of the power they had enjoyed earlier. Bloch mistakes the implications of the aristocratic model of marriage that subordinated the individual to the family but empowered daughters, sisters, and widows as family agents, an advantage they lost in the lineage-conscious twelfth century. Ecclesiastical emphasis on marital consent, however, weakened women's claims to share the material possessions of either family. When women could dispose of real power, as in tenth-century Italy, they were deprecated; as their power diminished, they could be relegated to the pedestal.

Bloch's contention that both virginity and love are destroyed by male gossip is a powerful one. Women's aspirations to virginity and to romantic love are the targets of misogynists because they threaten to undermine male domination of women and male solidarity with men. Many men do not like virginity and want to despoil it as an expression of female integrity. Similarly, men often despise the love that binds men to women and turns them from one another. Bloch's



intentions are clearly good, but his method betrays his intention. Substituting abstractions and *topoi* for conscious authors results in the absurd implication that Marie de France was the principal "essentializer" of the courtly lady. If we are to understand misogyny and uproot it, we must first acknowledge that it is the product of active and thinking men and then seek to understand the reality of woman's agency in dialectical relationship with man's reactive misogyny.

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RICHARD W. UNGER. *The Art of Medieval Technology: Images of Noah the Shipbuilder*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xv, 167; 77 plates. \$45.00.

Richard W. Unger takes a broad view of the relationship between art and technology in his book on the art of technology and illustrations of Noah building the ark. He discusses the questions of where artists obtained their ideas, their reliance on tradition, their sensitivity to technological change, and their sense that both artists and technologists were doing the same thing. Art and technology were intertwined in the medieval period in a way that they have not been since the Industrial Revolution. Artists were also influenced by the symbolical views of medieval Christianity that each story in the Bible typified certain values, that Noah illustrated obedience to God's will in his building of the ark to the Lord's specifications, and also that he was a precursor of Jesus. Pictures are likely to be more informative than technological treatises in the early period, when the latter tend to be obscure and difficult to understand. Artists were influenced by the view of the early Christians who could not see manual work as contributing to salvation. Not until after the Gregorian Reform had established a greater separation between clergy and laypeople did work become more important, and so did craftspeople. Thomas Aquinas gave the mechanical arts a lower status because of their purpose and also because of the low social status of the people involved in their practice. Nevertheless, financial developments led to the increased status of crafts guilds, which contributed to church windows and became important in town life and government.

All of these developments have been shown in the pictures Unger has found of Noah building the ark. He has discovered seventy-seven of these illustrations of Noah produced between the third and seventeenth centuries. Almost all depict Noah building the ark, at first doing the work himself with tools. Here Unger mentions the change in tools, like the introduction of the breast augur. As customs in Mediterranean countries changed, Noah is shown with a plan, explaining to professional shipbuilders what he wishes done. Because of the demand for the carrying trade in that

region, it was worthwhile for specialists in the trade of carrying people and goods from one port to another to build ships, if necessary to another person's requirements, and then to sail the ships wherever that person wished. At this time illustrations show Noah with a long sheet of paper detailing those requirements.

Unger describes the difference between skeleton and shell building, with shell building predominant in the north and skeleton building predominant in the south. Shell building consists of constructing the outside first with planks carefully overlapped and made watertight with pitch. In skeleton building, the frame comes first and then the outside is built. This method saves on construction time, is stronger, and weighs proportionately less. Eventually all ships were built this way.

The full-rigged ship is skeleton-built with three masts, each with a single sail. The main mast, the largest one, carried the square major driving sail. The stern mast had a lateen or triangular sail, while the small foremast balanced the mast at the stern to help in maneuvering. Such a ship was being built by at least 1440, and was used by Basque pirates and was also suitable for sailing across the Atlantic.

Unger has written a careful history of shipbuilding and does not appear to have any thesis. He can be criticized for his references to other writers, which are so short as to be difficult, if not impossible, to identify. The book makes an effort to absorb the material in understandable form but is perhaps too long. Nevertheless, my criticisms are minor. Unger has made a valuable contribution to the history of shipbuilding from the early Middle Ages through the seventeenth century.

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MARIA LAHAYE-GEUSEN. *Das Opfer der Kinder: Ein Beitrag zur Liturgie- und Sozialgeschichte des Mönchtums im Hohen Mittelalter*. (Münsteraner Theologische Abhandlungen, number 13.) Altenberge, Germany: Oros. Pp. xii, 490.

This work in religious studies treats child oblation in the early Middle Ages, focusing on the tenth and eleventh centuries. Maria Lahaye-Geusen acknowledges, and essentially rejects, the social interpretation of this phenomenon proposed by John Boswell in *The Kindness of Strangers* (1988), which apparently she is unaware of. She is much closer in method and spirit to a Dutch study of oblation in the early Middle Ages published by Mayke de Jong (*Kind en Klooster in de Vroege Middeleeuwen* [1986]). Lahaye-Geusen insists that the presentation of children to monasteries can only be truly understood as a religious phenomenon, and that at the heart of it is a sense of sacrificial offering. She takes as her chief source the customar-

ies that regulated monastic ritual and institutional life, which Dom K. Hallinger's series has now made readily available to scholars (*Corpus consuetudinum monasticarum* [1963- ]).

Drawing occasionally on concepts found in religious phenomenology, she reviews and interprets at length the provisions and rituals specified in the customaries. She describes how their provisions regulate and lend religious meaning to those young boys' entrance into the cloister, their "learning by doing" in the context of daily monastic prayer, and their upbringing in a carefully guarded moral innocence that was to be preserved from worldly admixtures. For Lahaye-Geusen, this practice, so central to monastic recruitment in the early Middle Ages, must be understood as a ritual offering up of children to the Lord, children who were then reared to assume their allotted position in society as spiritual warriors. The book is well structured and will probably prove most useful as a handy encyclopedia for all matter of questions pertaining to childhood, education, and religious ritual in early medieval monasteries. Her insistence on the religious aspects of this social practice is justified, but effectively one-sided—she would argue to redress the balance. Joseph Lynch demonstrated long ago (*Simoniacal Entry into Religious Life* [1976]) that medieval families were intensely preoccupied with all the social and economic elements entailed in presenting their children to monastic houses. Readers should approach this important and intriguing religio-social custom from other angles as well, including the legal angle, as indeed Lahaye-Geusen presumes they will. Her contribution here is to set out in detail and empathetically the religious dimensions of child oblation conceived as a sacrificial offering to God.

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BENJAMIN ARNOLD. *Count and Bishop in Medieval Germany: A Study of Regional Power, 1100–1350*. (Middle Ages.) Philadelphia: University of Pennsylvania Press. 1991. Pp. xiii, 212. \$32.95.

Historians have been accustomed to analyze political power on the basis of the nation-state, the most visible form of such power since the late eighteenth century. But when they follow this practice in the study of the European Middle Ages, they impose notions on their subject that do not always fit the evidence and can impede genuine understanding.

This is particularly pertinent for the study of the German Middle Ages. Imbued with ideas of nationhood developed in the nineteenth century, and faced with the seemingly solid evidence for French and English medieval monarchical structures, scholars had viewed the crisis of German monarchical power in the thirteenth century with acute dismay, labeling the emerging local structures "regional chaos."

Not so Benjamin Arnold in his new book. Arnold belongs to a growing number of medieval historians who feel that a closer look at regional structures would result in greater understanding of the realities of political power in medieval life. German regionalism," he writes, "was not a negation, the supposed failure of kingship . . . but the strength of an entrenched elite of princes . . . sharing power admittedly with . . . rivalry and intrigue, with the crown, ecclesiastical institutions, and . . . towns. The . . . vital nature of German regionalism . . . makes it difficult to write coherent German history except in regional terms" (p. 28).

Arnold's book concerns the political, economic, social, and military structures of a single region, the bishopric of Eichstätt, situated across the border between Bavaria and Franconia. He focuses his discussion on the end of the thirteenth century, when the waning presence of the crown in local affairs brought regional rivalries into sharp relief. In Eichstätt, the bishops began to reassert their secular powers not only against count-advocates and knights who were technically their vassals and *ministeriales*, but also against the looming princely power of Bavaria and the growing strength of both the burgrave and the city of Nuremberg.

In five detailed chapters, the author shows how the region functioned politically, how the various bishops kept their independence from their mighty neighbors, and how, through the accidents of birth and death and political inaptitude, they managed to eliminate the power of two aristocratic families, the Count-Advocates of Hirschberg and the Counts of Oettingen. The Prince-Bishop of Eichstätt remained a strong regional presence until the secularization of the ecclesiastical principalities in 1802.

Arnold's investigation presents a major contribution to the study of German medieval history. His chapter "Regions and Power in Medieval Germany" is a masterwork of historiographical exploration. The book is less successful, however, on one point. In the conclusion, Arnold asserts that personal charisma and ability were primary factors for success in the exercise of political power during the Middle Ages. Unfortunately, the portraits of the Eichstätt bishops, to say nothing of their adversaries, remain shadowy, without the vivid detail that Arnold brings to his study of fiefs and castles. To be fair to the author, this is perhaps the fault of his sources. Medieval narratives can be sparse on personal details. The existing Eichstätt biographies could have been explored more fully, however. My own recollection of an Eichstätt ecclesiastic is of Bishop Michael, whom the sources described as warlike. In August 955, he came to the aid of Bishop Ulrich of Augsburg in the Magyar siege of that city, part of the Battle of the Lech, and he died a hero's death there.

Be that as it may, Arnold's book is most impressive

and should become a "must-read" for all students of medieval Germany.

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CONSTANCE BRITTAIN BOUCHARD. *Holy Entrepreneurs: Cistercians, Knights, and Economic Exchange in Twelfth-Century Burgundy*. Ithaca: Cornell University Press. 1991. Pp. xiv, 242. \$33.50.

The heartland of Cistercian life in the twelfth century was Burgundy, but the twenty Burgundian houses of men have been relatively neglected in modern scholarship. Two-thirds of their surviving charters are unpublished and even the cartularies of Clairvaux and Cîteaux are incompletely edited. Consequently, much about Cistercian life in its earliest and presumably purest form, especially economic life, is unknown.

The dearth of solid information about Burgundian Cistercian economic practices aggravates a modern conceptual problem. The economic success of the Cistercians in the twelfth century has posed problems for modern scholars, who implicitly or explicitly see shrewd economic management as incompatible with holiness. When scholars find sophisticated economic practices in studies of Cistercians in other regions, they sometimes attribute them to local "decline" and imagine that things were purer at Clairvaux or Cîteaux.

Constance Brittain Bouchard has challenged that view in this revisionist book. On the basis of her examination of about 2,300 charters from Burgundian houses, she demonstrates the broad participation of the Cistercians in the rising economy of the twelfth century. She argues that the rapid expansion of the Cistercian order was inconceivable without a solid economic base. Gifts of land constituted the core of Cistercian economic life, but gifts alone rarely yielded the compact granges so central to Cistercian agricultural practice. Bouchard demonstrates that without embarrassment the Cistercians used pawns, leases, exchanges, and purchases to rationalize their holdings.

The author criticizes directly the modern uneasiness with any link between holiness and financial success. She argues that diverse Cistercian economic activity began very early and is not a sign of decline but of good management. Modern scholarship has generally accepted the *Exordium parvum* and the so-called Legislation of 1134, which forbade possession of certain kinds of property, including churches, tithes, and peasant holdings. Bouchard argues, not entirely convincingly, that those texts must not be understood as the expression of the primitive Cistercian ideal, but rather as an attempt to respond unsuccessfully to problems of a later generation, perhaps the 1150s.

The Cistercians did make claims for their uniqueness: they observed Benedict's Rule *ad litteram*; they simplified their liturgy and their diet; they and their *conversi* worked with their hands; they avoided urban life and lived in the "wilderness." But Bouchard argues that the Cistercians did not generally claim that their uniqueness was bound up with particular forms of property and economic behavior. The charters show that before and after 1134, Cistercian houses in Burgundy did own such "forbidden" properties. Indeed, she demonstrates that their holdings in land, usage rights, exemption from tolls, rents, and even churches and tithes were more diverse than has been traditionally thought.

A signal strength of this book is the author's care to show that contemporaries understood and expressed in the charters the different transactions in which a monastery might engage. There was no confusion among pawns, leases, purchases, and gifts. In particular, Bouchard argues that modern attempts to find hidden motives for pious gifts are anachronistic: "Property specified as free gifts in alms should be so considered" (p. 78). The small counter gifts that occasionally accompanied gifts were not disguised purchase prices but expressions of the friendship created between donors and the monks.

Bouchard also stresses the importance of knights in the growth of Cistercian monasteries. In the tenth and eleventh centuries, the Black monkhouses found their chief patrons among the highest aristocracy. The rise of the Cistercians coincided with the consolidation of the knightly class and it was from that group that the Cistercians drew much of their support. The same group was also the source of enemies and litigation. Because the knightly class was not as rich as the high aristocracy, some of their heirs wished to get back what their ancestors had given. The Cistercians spent much time defending their varied properties against the claims (*calumniae*) of their knightly neighbors.

In addition to being an important revisionist study of Burgundian Cistercian economic practices, this clear book is an excellent brief introduction for anyone wishing to understand twelfth-century charters and cartularies.

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BERNARD GUENÉE. *Between Church and State: The Lives of Four French Prelates in the Late Middle Ages*. Translated by ARTHUR GOLDHAMMER. Chicago: University of Chicago Press. 1991. Pp. 447. \$39.95.

Bernard Guenée begins his book with a discussion of biography and history from ancient times through modern French biography, the reversal under the *Annales'* concentration on the large picture and quantitative compilations, and now the move again toward

the qualitative and individual. He starts with the medieval French church's relations with the king and moves on to consider four prelates who had been caught as both churchmen and Frenchmen during the period from Louis IX to the start of the Italian wars, when the king became more powerful and the church more French, and when political loyalty assumed the mantle of orthodoxy and treason became like heresy. The figures he chooses to focus on are Bernard Gui (1261–1331), Le Muisit (1272–1353), Pierre D'Ailly (1351–1420), and Thomas Basin (1412–90). They were major actors (D'Ailly), outspoken witnesses (Basin), or more modest figures (Gui and le Muisit). None were men whose family established their place; all came from good families, which gave them a chance to rise, but climbed because they had talent, acquired knowledge, and lived long enough for experience and contacts to count. Guenée examines how their careers illustrated general ideas but does not give equal space to each: Gui takes 64 pages, le Muisit 30, D'Ailly 156, and Basin 116 pages; so the last two require three-quarters of the book, while the first two tell what the world was like in which D'Ailly and Basin set out to make their mark.

The four were quite different: Gui was a Dominican from near Limoges, served in the early 1300s as Inquisitor for Toulouse, wrote history, became a bishop, and left a manual for inquisitors and hagiography. He was a faithful adherent of the status quo. Gilles le Muisit was from the border city of Tournai, entered the monastery of Saint Martin there in 1289, had probably studied at Paris, and developed a taste for history and French poetry. Later he rose to prior and abbot and became blind from cataracts before an operation cured this. He viewed the many terrible events in his lifetime as punishments for men's sins—rebellions, wars, famine, defeat of France, plague, his own blindness, the massacre of Jews and the Flagellant movement—and yet his observations on all this were very circumspect.

D'Ailly went off to Paris to study while young and stayed for fourteen years, so the city and university were at the heart of his love and interests for life. At the College of Navarre he made connections and found a patron. In the major crisis of his age, the Great Western Schism, the roles of the University of Paris, the king of France, his relatives (the dukes of Berry, Burgundy, Orleans), and D'Ailly's own attempts to resolve the crisis all became linked. D'Ailly was a prolific writer and respected orator, preacher, and debater, a member of a circle of friends taking in the disputes that divided both the church and the kingdom and that turned bloody. D'Ailly became prince-bishop of Cambrai and later a cardinal who played a leading role at the Council of Constance, which ended the schism, but he was by then an exile from Paris and its circles.

Basin, a man of the next generation, was from Normandy and shared that province's sense of special identity. He became bishop of Liseux at the age of

thirty-five and seemed to have arrived, but when, in 1461, he crossed the Dauphin (who soon became Louis XI), his career was ill-fated. He spent his last decades as an outsider and exile. He composed his own apology and a history of the reigns he had witnessed, following ancient Latin historians whom he admired and not attempting an impartial, objective presentation of the events.

These four lives give an opening into the great events of their day and the movements that were changing their world. There are naturally some differences of interpretation and an occasional slip. The relationships of the Beauforts and Henry V and VI are confused (p. 290); November 1, not November 2, is All Hallows' Day (p. 71). The assertion that the election of Urban VI in 1378 was perfectly regular belies 600 years of controversy on its validity (p. 119). The few unfortunate typos do not detract from the value of this book, which stands with the recent works by R. W. Swanson (*Church and Society in Later Medieval England* [1989]) and R. Haines (*Ecclesia Anglicana* [1989]) on the English church and churchmen of that same era in filling in what has been a very dim image.

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JOHN BLAIR and RICHARD SHARPE, editors. *Pastoral Care before the Parish*. (Studies in the Early History of Britain.) New York: Leicester University Press; distributed by Columbia University Press. 1992. Pp. 298. \$59.00.

Until recently the early medieval ecclesiastical history of the British Isles replicated the nationalism of modern history: differences between the Irish and English churches, for instance, were a given. This book edited by John Blair and Richard Sharpe is directed against "national church" approaches and diminishes many time-honored contrasts. It is divided into two sections, one on Celtic Britain and Ireland and one on Anglo-Saxon England. An excellent introduction by the editors gives an overview of the history of scholarship and lucidly draws out some of the main contributions of the individual chapters. In general the book's contributors see pastoral care in all the areas under study as based on mother-churches. The book tends toward a more favorable assessment of the quality of Anglo-Saxon pastoral care than most earlier historians have allowed.

The first chapter, by Steven Bassett, deftly uses topography in arguing that the Anglo-Saxons found an active British church when they arrived in the West Midlands. A British diocese may be the origin of the minster *parochia* of Worcester. In the second chapter, Huw Pryce argues that from the early Middle Ages monastic communities in Wales included both secular and monastic clergy. The saints' cults in particular reveal lay devotion. Thomas Charles-



Edwards next discusses the "pastoral role of the church in the early Irish laws." The *Hibernensis* is shown to distinguish sharply between monastic and clerical churches. There is evidence of pressure coming from the laity for pastoral care. Prescriptive texts such as those studied by Charles-Edwards raise the problem of how much they reflect practice. Sharpe follows by looking at other kinds of evidence and performs the valuable service of showing "that expectations of pastoral care were to a considerable degree fulfilled" (p. 81). Sharpe attacks at almost every point the episcopal-monastic antithesis as found in Kathleen Hughes's work. Both he and Charles-Edwards see in the head church of the *tuath* ("people") the Irish equivalent of the minster. Pryce sees the Welsh mother-church possessed of pastoral functions like those found in England; Alan Macquarrie, examining the foundation of religious houses in Scotland, sees the Scottish mother-church similarly.

In the second section, Alan Thacker writes on "Monks, preaching and pastoral care in early Anglo-Saxon England," emphasizing the place of the *monasteria* in pastoral work, but seeing the minsters as varying greatly in size, status, and manner of life. Thacker's special concern is to remedy a gap in Bede, exhibiting an interest in the impact of the church on ordinary men and women. Sarah Foot contributes two chapters, the first showing the importance of baptism in the life of the laity. Her second chapter reviews terminology, and she proposes that "minster" is the best word to designate Anglo-Saxon religious houses before the tenth-century reform. Catherine Cubitt dissents to some degree from what tends to be the new orthodoxy elsewhere in the book, and she points out problems with the hypothesis of some of her colleagues. She argues that the association of pastoral work with bishops and priests and not with (unordained) abbots and monks in the canons of the 747 Council of *Clofesho* is not simply an episcopal perception. More attention should be given to the distinction between clerical and monastic houses and to the variety of traditions introduced into the Anglo-Saxon kingdoms from elsewhere. Blair gives a topographical review of Anglo-Saxon minsters and suggests developments common to northwest Europe. Finally, Gervase Rosser, in studying pastoral care from 700 to 1000 in English towns, argues that ninth and tenth-century town-based evangelization has been underestimated. This is a valuable collection of essays that opens many paths for future work.

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PETER COSS. *Lordship, Knighthood, and Locality: A Study in English Society c. 1180–c. 1280*. (Past and Present.) New York: Cambridge University Press. 1991. Pp. xv, 361. \$59.95.

The archives of great ecclesiastical estates have determined the shape of most socioeconomic studies of medieval England. Peter Coss's enterprise rests in contrast on a decade's hard study of converging deed collections assisted by heavy use of common-law plea rolls. An *Annales*-type regional study of the Midlands town of Coventry and its Warwickshire environs, this book depicts the prehistory of the classic English gentry and of "county society in a real sense" (p. 324).

Coss begins with a nuanced account of the local context within which landed players operated. A single organic system comprising lords, vassals, and neighbors in both town and country overspilled county bounds. There was as yet no county community. Center-stage once again is lordship, responding to crisis pressures in a manner "as rational and as managerial as could be desired" (p. 83). Warwickshire estates tended to be rent complexes with little *demesne*, hence with minimal incentive for conventional "High Farming." Lords competed for resources to maintain family living standards against inflation. Proto-gentry needed to restructure as best they could within the space left by their betters. Some succeeded; others spiraled into debt and out of contention.

Crucial in "a tense and problematic society" (p. 205) was management structure and control. The key to success was "social prestige," especially knighting, which enhanced reputation and tenant subservience, "the real guarantors of income" (p. 272). The decline in knightly numbers, exhaustively calculated, establishes a genuine crisis in the decades before 1258. The ensuing shake-out roughly halved the number of families; the core of the future gentry remained. Thereafter demands from both king and aristocracy, expressed especially through parliament, reshaped county society into genuine community and "a new polity."

Although not all of this is new, Coss's case, subtle in its detail and convincing in its scholarship, is much more persuasive than his predecessors'. Even in this essentially expository appraisal, his premises deserve notice. His actors are rational in notably post-Post-anian ways (see, for example, pp. 148–49). He prefers structural explanations, even (unfashionably) economic ones, and assumes crisis and tension as an explicit corrective to previous literature. Much hangs on the "Great Inflation." That worries this skeptic, since the literature patently miscalculates its annual rate, offers a superabundance of causation, and yet cannot demonstrate its effect beyond 1180–1220. He stresses the effects of the Angevin law reforms, and most laudably hauls the new Milsomian orthodoxy into mainstream critical consideration.

There is more to say here, and I am sad that my own relevant publications do not come into play, doubtless because of their location. The need for interspecialist dialogue seems particularly acute here; Coss's relative discomfort with legal materials is occa-



sionally discernible, as in his misclassification of a routine entry *ad terminum qui preterit* as "odd" (p. 68).

Independent-minded social historians of Coss's technical caliber are sadly rare in the United States. His fine, ambitious book should transform not merely English social history in the broadest sense but also important aspects of our understanding of the medieval West as a whole. Like the bastard feudalism debate engendered en route, it also positively challenges readers to disagree.

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L. R. POOS. *A Rural Society after the Black Death: Essex, 1350–1525*. (Cambridge Studies in Population, Economy and Society in Past Time.) New York: Cambridge University Press. 1991. Pp. xv, 330. \$59.50.

This is an exemplary study of a portion of northern and central Essex in the fourteenth and fifteenth centuries. Spread over some 500 square miles, encompassing some 20,000 people, the district included only one genuine urban center, Colchester. It was otherwise almost entirely rural.

Presented in the guise of a monograph and written in the clinical and impersonal style favored by demographers, this book is much, much more than a local study, L. R. Poos's self-effacing protestations notwithstanding. It will become required reading for historians of both medieval and early modern Europe. This emphatic declaration may be a reaction on my part to Poos's decidedly unemphatic presentation. Let the reader judge.

This book is important because Poos asks important questions. The chief of these is: how different was rural society in the fourteenth and fifteenth centuries from what it was to become some 200 years later? Not very different, is the answer unambiguously provided here, in spite of all the inevitable qualifications concerning the fragility of the evidence. For instance, it is not really possible to determine conclusively what the average age at marriage was in rural England before 1500. Nevertheless, the author is able to identify the familiar outlines of what we think of as early modern social structure in this densely populated corner of Essex even before 1348. In the fifteenth century, far from conforming to the standard image we have of medieval village society, here "the world that country folk knew as theirs was not so very alien from the world familiar to their descendants of 100 or 200 years later" (p. 290).

Among the signs of this precocious development, the author points to an extremely mobile and highly stratified rural population composed in large part of land-poor wage earners; a genuine rural industry driven by export demand; patterns of marriage and household formation that resemble those of the later period; and the beginnings of literacy and religious

nonconformity among the middling sort of rural Englishmen.

Some of these conclusions are, of necessity, tentative. They are persuasive, in the aggregate, because both the author and his reader are in possession of a template of sorts. Knowing what the complete puzzle will look like around 1600, they can see the internal logic of the fragments assembled here. The author, conscious of the surprises conjured up in his chosen corner of England, admits that Essex may not be England and calls for "a new generation of regional studies . . . to situate the district's experience within a national context" (p. 293).

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I. M. W. HARVEY. *Jack Cade's Rebellion of 1450*. New York: Clarendon Press of Oxford University Press. 1991. Pp. xiv, 220. \$59.00.

In spite of the growing volume of late-medieval scholarship, the motivation and impact of Jack Cade's rebellion have not been explained. We are even unsure of Cade's identity. I. M. W. Harvey attempts to paint an impressionist landscape of this important but enigmatic revolt. He has carefully examined the records of the King's Bench to provide an array of dark points in the anecdotal picture of pervasive corruption and disorder in Kent and the Southeast that Roger Virgoe's published selection of indictments only sketches (*Documents Illustrative of Medieval Kentish Society* [1964]). Although Harvey enriches his palette through the evidence of popular ballads, the final result is mixed. At times Harvey fails to connect the isolated anecdotes to produce a clear outline of persons and events; had he used the works of Barbara Hanawalt, he might have provided a methodological focus for the criminal evidence. Harvey's overall perspective, however, is clearly conveyed: "It would be a gross anachronism to suggest that by the mid-fifteenth century there existed an expectation of fair and impartial government . . . but there was evidently a level of unfairness and partiality which was acceptable and one which was not" (p. 185).

Unacceptable to whom? Harvey has followed Ralph Griffiths's caveats on the use of the pardon roll of July 1450 as a guide to the rebel host (*The Reign of Henry VI* [1981]). Instead, a sufficient number of anecdotes are placed on the canvas to present a convincing picture of a revolt supported by a social group below the aristocracy and the gentry. These lesser men were already vital to local political and economic life in a shire such as Kent that had no strong aristocratic lordship; in 1450 they were able to articulate broader concerns. Harvey broadens his picture of this class with glances at Lollardy and at the cloth industry, but these efforts require more detailed brushwork. The

economic aspect of the revolt would seem especially promising for further research.

Griffiths provided the best account of the actual revolt and its political implications, but Harvey has added color and detail. Yet the basic questions of who Cade was and what his ties were to the Duke of York remain unanswered. Harvey gives much information on how Henry VI dealt with Kent in Cade's wake. Yet why Kent would refuse to rise for York in February 1452, only to then rise in May when it was too late, is again not fully explained. Kentishmen had their own agenda, as Harvey suggests, and they had been harshly treated since their revolt, but they did rise in May. Harvey might credit Henry VI's rare burst of energy, which saw him enter Kent only twenty-four hours after York, as a partial reason why York's hopes were forestalled.

This volume provides valuable information on the landscape of Cade's rebellion; the portrait of that rebellion still remains unfinished. Price aside, Oxford has produced an attractive book with useful maps and bibliography.

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A. LONDON FELL. *Origins of Legislative Sovereignty. Volume 4, Medieval or Renaissance Origins? Historiographical Debates and Deconstructions.* New York: Praeger. 1991. Pp. xviii, 492. \$49.95.

A. London Fell has produced his fourth volume on the origins of legislation in Western European thought in what he projects will be an eight-volume series. The first two volumes dealt with Corasius, a French jurist who presided over the trial of Martin Guerre. He treated the thought of Jean Bodin in the third volume. In the present volume Fell discusses the work of medieval and Renaissance historians who have written about sovereignty, legislation, and the state.

Fell's first three volumes were sprawling essays whose chief virtue derived from their presentation of Corasius's thought. Their vice was lack of organization, indexes, and a good editor. The theme was that Corasius influenced Bodin and that the two jurists held a conception of sovereignty that broke decisively with medieval juristic thought. In the first three volumes, Fell repeatedly claimed that "medieval political theology" had been rejected in the sixteenth century, but he never quite established what medieval political theology was.

This volume attempts to answer that question. He concludes that medieval jurisprudence did not have a doctrine of legislation, did not recognize the territorial state, and did not have a conception of sovereignty. Fell "deconstructs" the three terms—legislation, sovereignty, and state—and demonstrates to his satisfaction that medievalists have consistently mis-

used these words by anachronistically attributing modern ideas to medieval and Renaissance jurists.

It is difficult to find something to praise in this volume. The upper and lower levels of the text are disconcertingly disconnected. Fell first reviews the works of selected medieval and early modern historians of political thought. The remainder of his book consists of notes that are filled with long quotations from the works he discusses, interspersed with his own comments. The resulting "historiography" is far from being a coherent analysis of the works he cites. In a chapter on "The Florentine State" he discusses the work of Hans Baron at some length, mentions Donald Weinstein, William Bouwsma, J. G. A. Pocock, and others in passing. The endnotes for this section are a subterranean discussion with little "reviews" of books and articles.

To his credit, Fell has read extensively and cites much literature, but the lack of an index is exasperating. He apparently has overlooked many fundamental works or relegates them to passing reference. Yet he often discusses older works (pre-World War II) at great length. In his discussion of Baldus de Ubaldis he omits Joseph Canning's book on Baldus (*The Political Thought of Baldus de Ubaldis* [1987]), and he submerges Ennio Cortese's indispensable volumes to a footnote (*La norma giuridica* [1962–64], p. 444).

His concentration on older historiography is understandable. Fritz Kern, to whom he devotes much space, did not think that legislation existed in the Middle Ages. Fell cannot cite any postwar historian of the *ius commune* who thinks that medieval jurisprudence did not develop both the theory and the practice of legislation. Few medievalists would argue that a medieval "prince" who promulgated laws understood the legislative process as we do today, but medieval jurists understood that the prince could change, derogate, abrogate, or promulgate law. Fell overlooks the fact that Kern's evidence is almost entirely drawn from the early Middle Ages, when there was not much if any legislation in the modern sense of the word. Raoul Van Caenegem has recently pointed out that neither kings nor princes nor popes promulgated new legal norms during the tenth century (*Cambridge History of Medieval Political Thought* [1988], 181). Several reasons might be given for this, but the most important is undoubtedly that monarchs could not justify legislation with a commonly accepted legal doctrine.

One could cite hundreds of passages taken from the jurists of the *ius commune* between about 1150 and 1500 in which they state that "the king (emperor, pope, or prince) has the authority to promulgate laws." To cite just one example: Marinus de Caramanico (ca. 1275) wrote: "unde sicut imperatoris, ita regis est proprium condere legem" (Just as the emperor has the authority to promulgate law, so does the king). If all those passages mean something quite different from what they literally say, Fell should have presented an argument for his interpretation in

this volume. Instead, he dismisses the work of many scholars with a wave of his hand and without ever presenting his reader with the evidence that justifies his position.

Fell indicts most of the historians he considers in this volume with the charge that they do not understand the terminology of medieval or modern jurisprudence. A test of his own understanding might be how he defines "legislative sovereignty" in the title of his book. Unfortunately, after four volumes, Fell has yet to define the term. He did define the "legislative state" of his title as "an autonomous entity based on a body of enacted laws" (vol. 3, p. ix), but it is difficult to know how he can defend such a definition in the 1990s. The political realities of European integration should have forced him to conclude that either legislative states no longer exist in much of Europe or that his definition is in serious need of repair.

One can say that Fell has enormous enthusiasm for his project. His subject is important. But his question of whether the state and legislation existed in the Middle Ages reflects the concerns of a much older generation of scholars. Most legal historians have decided that the answer is not simply yes or no. Medieval jurisprudence is not modern, yet we should be able to discuss what they thought about law using terms that they themselves used. They had legislation, sovereignty, and state in their vocabulary; without question, these words did not mean what they mean today. This should neither surprise nor dismay us. If we restricted our terminology to words that meant then exactly what they mean today, discourse about the past would become painful, ugly, and short.

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#### MODERN EUROPE

PHILIP D. CURTIN. *Death by Migration: Europe's Encounter with the Tropical World in the Nineteenth Century*. New York: Cambridge University Press. 1989. Pp. xix, 251.

Philip D. Curtin's book makes important contributions to at least three fields: the history of medicine and public health, historical demography, and the history of European colonialism. Curtin has performed the valuable and laborious task of analyzing British and French military medical records from 1815 to 1914 in order to arrive at some significant conclusions regarding what he calls the "relocation costs" of European imperialism. At the beginning of the nineteenth century, relocation costs, or deaths from disease of British and French soldiers stationed in the tropics, were at least twice that of troops who remained in Europe. Yet by the first decade of the twentieth century, mortality among soldiers both at home and abroad had declined by 80 percent and more. Using quantitative data from the British West Indies, Algeria, and Madras, India, each of which

presented its colonizers with distinct health problems, Curtin documents patterns of mortality and morbidity among soldiers and explains changes in those patterns during the course of the nineteenth century.

In the first part of the book, covering the period to 1860, Curtin examines causes of death in Europe and the tropical world. In Europe, for example, more than half of all deaths were caused by diseases of the lungs, especially pulmonary tuberculosis. In the West Indies, malaria and yellow fever were the leading causes of death, while in Madras, waterborne infections such as cholera, typhoid, and dysentery posed the greatest threat to colonists. In Algeria, however, military campaigns accounted for much of the mortality, although malaria and typhoid also played a role. Whereas relocation costs were high at the beginning of the nineteenth century, mortality rates among European soldiers stationed in the tropics declined dramatically, in some areas by as much as 90 percent, between 1840 and 1860. According to Curtin, public health practices such as improved sewage disposal, ventilation, and access to clean water accounted for much of this change.

The second part of this study, examining the years 1870 to 1914, coincides with the emergence of modern medical science largely as a result of the discovery of the germ theory of disease and the systematic dissemination of medical knowledge. During this period, Curtin finds that while empirical advances continued to improve the health of Europeans overseas, disease prevention, in the form of immunizations (typhoid inoculation), and direct intervention in the course of diseases (rehydration in the case of cholera and the use of ipecac for dysentery) had the most profound impact on mortality rates. The numerous statistical tables that appear in the appendix are especially useful for understanding disease and mortality patterns after 1860.

Curtin plans to use much of this same documentary evidence to undertake two more important projects: an analysis of patterns of mortality among non-European troops in these same three areas and a study of disease and mortality among native soldiers transported from their homelands. Historians and demographers alike can look forward to the publication of these two studies.

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IRENA GRUDZINSKA GROSS. *The Scar of Revolution: Custine, Tocqueville, and the Romantic Imagination*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xv, 191. \$29.95.

This slim but intriguing book brings together two French figures from the early nineteenth century, Alexis de Tocqueville and Astolphe de Custine. Tocqueville visited Jacksonian America in 1831–32 and his *De la démocratie en Amérique* (1835) stands as

perhaps the most perceptive analysis of American society ever written by a foreign observer. Custine is now far less well known. But his work, *La Russie en 1839* (1843), remains a highly regarded nineteenth-century study of Russian society and culture.

In her book, Irena Grudzinska Gross presents a thoughtful comparison and contrast of these two writers and their accounts of America and Russia respectively. She examines their personal backgrounds and sets the two men in the historical and cultural context of postrevolutionary French society. She offers a study of the history of European culture by carefully exploring the various sources—personal, social, literary, and historiographical—of Tocqueville and Custine and by focusing on the reactions of these two travelers to the nations and peoples they visited. Despite these larger purposes, the book will strike many readers as a work of literary criticism (complete with several examples of precise *explications de texte*) rather than a work of cultural history more broadly construed.

The author deftly indicates similarities between Tocqueville and Custine. Contemporaries of aristocratic background, both were profoundly influenced by family experiences during the French Revolution and by the literary and cultural currents of romanticism, especially as personified by Chateaubriand. Both felt they were to some degree outsiders, or men “in-between,” and both wrote important books of commentary about a foreign society in part to explicate the French situation, to suggest solutions for contemporary problems, and to explore what might become of postrevolutionary France and Europe. She also effectively indicates a major contrast. Whereas Custine presented a thoroughly negative image of Russia as a place of violence, fear, injustice, and tyranny, Tocqueville found in his American experience some degree of hope for the future.

As a contribution to the study of Tocqueville, the book reflects François Furet’s interpretation that Tocqueville arrived in America with questions already framed in France and that his reactions to the New World republic were largely shaped by the French experience. Gross underscores the influence of romanticism and, specifically, of Chateaubriand on the emotional tone of Tocqueville’s perceptions and responses to America. She joins those who see Tocqueville as fundamentally a nineteenth-century figure rather than as a man of the eighteenth (or even seventeenth) century. Tocqueville’s lucidity and his larger patterning after Montesquieu are conceded, but his poetic, often melancholic, stance is emphasized as a reflection of Chateaubriand and the romantic generation.

Although this viewpoint is instructive, it does not sufficiently recognize that much of Tocqueville’s brilliance and originality was precisely his ability to move beyond the romantic heritage that, for example, trapped his traveling companion, Gustave de Beaumont, when he wrote his work on America, *Marie*.

Gross also makes too much of small portions of Tocqueville’s writings on America. Indeed, the Tocqueville section of the book focuses not on *Democracy in America* or even on the bulk of his travel notes, but on various texts and accounts—by both Tocqueville and Beaumont—of a single episode of his American experience, the journey to Lake Oneida. The author’s case, while interesting and suggestive, stands on too narrow a base. The book succeeds, however, in offering some wonderful insights about Tocqueville and his reactions to America and in introducing readers to Custine and his work on Russia. The book also includes an excellent index.

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PETER J. BRENNER. *Reisen in die Neue Welt: Die Erfahrung Nordamerikas in deutschen Reise- und Auswandererberichten des 19. Jahrhunderts*. (Studien und Texte zur Sozialgeschichte der Literatur, number 35.) Tübingen: Max Niemeyer. 1991. Pp. ix, 450. DM 136.

This work by Peter J. Brenner appears in a series dealing with the social history of literature, but it is of interest not only to the literary but also the social historian. It aims to explore the America-experience in nineteenth-century travel reports of German travelers and immigrants. Brenner deals with these publications in a typological approach, thus dividing them into three categories, those by immigrants, by adventurers, and by scholars.

The travel reports of immigrants vary from guidebooks with almanac-type data to extensive reports, some of which were glowing in praise, others of which contained critical commentary. The reports of the “adventurer” were more greatly influenced by literary works dealing with America, such as those by James Fenimore Cooper, Karl Friedrich May, and others. In the travel works of scholars specific interests emerged, such as ethnography and the classification of flora and fauna. These three types of reports approached America from a different perspective, but all contributed to the German image of America in the nineteenth century, which, especially early on, was important in the history of the German immigration to the United States.

Brenner examines the various elements of the German travel reporter’s experience of America. Among these are the Germans’ first contacts and initial information about America, their experiences in urban areas, their views of urban architecture, the contrasts between Yankees and Germans, the issue of slavery, and a wide range of other social, political, and cultural aspects.

An extensive bibliography guides the reader to the primary sources on which this work is based. Students of German-American history will find this work useful, although I would have preferred an annotated bibliography of the primary sources. Although Bren-



ner's typological analysis of German travel reports provides a basic understanding of the form and content of these reports, it does not provide us with a general history and evolution of these writings within a chronological framework, nor does it draw any general conclusions about the German image of America.

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DAVID HARRIS SACKS. *The Widening Gate: Bristol and the Atlantic Economy, 1450–1700*. (The New Historicism: Studies in Cultural Poetics.) Berkeley and Los Angeles: University of California Press. 1991. Pp. xxvii, 464. \$45.00.

Inclusion of this innovative economic and urban history in Stephen Greenblatt's series, "The New Historicism: Studies in Cultural Poetics," seems increasingly appropriate the further one reads in this well-written integration of economic, social, political, and religious themes. David Harris Sacks extends, expands, and distills work that appeared as his two-volume *Trade, Society and Politics in Bristol, 1500–1640* (1985) in order to explore Bristol's transition from a medieval commercial town to an early modern capitalist center.

For Bristol, the momentous event of 1453 was not the fall of Constantinople but the English surrender of Bordeaux; Bristol immediately lost its profitable trade in the wines of what had been English Gascony. Bristol's merchants, whose profits were persistently higher on imported luxuries than on exported cloth, lead, leather, or grain, soon found comparable imports from Iberian and Mediterranean ports. Spain provided the most profitable trade, despite periodic recessions and Anglo-Spanish wars.

Cloth became less significant among exports, replaced in part by fish from Iceland and Newfoundland. Bristol's merchants trading overseas adapted better than the rest of their city to the declining cloth trade and the commercial stagnation that characterized the first two-thirds of the sixteenth century. The Society of Merchant Venturers was organized in 1552 to consolidate the power of these successful merchants against local interlopers and to create a lobby in local and national politics. These "mere merchants" became a more distinct, powerful, and exclusive group in the subsequent century.

After ample theoretical introduction, Sacks explores the sociopolitical aspects of the period between the loss of Bordeaux and the English Civil War. Civic-religious festivals, ceremoniously proclaiming local unity, were reduced and transformed by the Reformation. Queen Elizabeth's royal visit of 1574 displayed a stronger taste for hierarchy, order, and martial preparedness. City leaders increasingly derived their power from the crown, and the two sources of legitimacy made political dissension more

likely. By the 1630s the Merchant Venturers were led by Laudians who won a new charter, strengthening their power against artisans and retailers, many of whom were religious reformers. Yet city politics did not fissure cleanly along these social or religious lines.

Perhaps there is no clearer demonstration of the impact of American trade on an English city than Sacks's section on the "Capitalism of the Spirit, 1650–1700." The exploding trade with the colonies was no "mystery" compared to other overseas trades. Kin, friends, and co-religionists provided contacts within an English social, economic, and legal empire. Not only were mixed cargoes of manufactures easily saleable but colonial demand for labor also seemed endless. Servants became a profitable export for lesser merchants, retailers, artisans, and mariners.

Bristol's well known *Register of Servants to Foreign Plantations*, accounting for some 10,000 emigrant indentured servants between 1654 and 1686, is here seen in an entirely new light. Its ostensible purpose, to curb the "spiriting" of hapless people from their families, was fallacious, since apprentices could be indentured without parental consent. Sacks argues convincingly that the register was a vain attempt to curb the servant traders who were reducing the Merchant Venturers' share of colonial imports. By forcing the payment of indenture fees in coin and adding intrusive inspections and heavy fines, the registration process was intended to disrupt this profitable trade of Bristol's lesser traders.

Although Sacks does not reexamine the servant list, he does comment thoughtfully on existing explanations for servant emigration. Sacks sees English economic factors as dominant from 1654 to 1662, but emphasizes Bristol's ferocious repression of sectaries, especially under Sir John Knight, mayor in 1663–64. Quaker and Baptist dissenters, however, proved too numerous and powerful to be crushed in Restoration Bristol. Sacks sees them as economic innovators who provided a new business ethic in a fractured community. Their religious communities not only provided partners, investors, and customers but enforced business prudence and honesty as well. Having lost in the political sphere, the sects sought to convert by example within the business community. Their new moral discipline provided needed security amid rapid economic change.

By way of conclusion, Sacks examines the world views of three Bristol writers. Roger Edgeworth's *Sermons* of the 1540s and 1550s described the merchant's life as morally risky and economically unpredictable, dependent on God. John Browne's *Marchants Avizo* of 1589 argued for prudence, trust, and flexibility within a dominant business community that recognized some place for providence. John Cary's essays of 1695 and 1696 revealed an Atlantic political economy, a mechanical system requiring only minor political adjustments. Hierarchical assumptions had disappeared and improvement of life was a clear objective. Perhaps there is a little sleight of hand in



using a preacher and then two members of the Merchant Venturers to plot the evolution of Bristol's view of economic life, especially since Sacks assigns a significant role to Restoration dissenters.

This thoroughly researched, multifaceted, and rewarding study is a model of modern scholarship unshackled from specialization.

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ELLEN MEIKSINS WOOD. *The Pristine Culture of Capitalism: A Historical Essay on Old Regimes and Modern States*. New York: Verso. 1991. Pp. x, 200. Cloth \$59.95, paper \$16.95.

In this lively "essay," Ellen Meiksins Wood, a historian of political thought, seeks to restore the picture of early modern England as a period of fundamental transition from feudalism to capitalism that is associated with the work of Christopher Hill and other British Marxist historians of the 1950s and 1960s. The essay is structured as a series of arguments, first with New Left writers Perry Anderson and Tom Nairn, who have traced modern Britain's inadequacies to the incompleteness of its transition from feudalism to capitalism, and then with two "Right" scholars, Jonathan Clark and Alan Macfarlane (if he can be so labeled), who have rejected the notion that these centuries witnessed any such radical transformation.

Wood's strategy to reestablish the sixteenth and seventeenth-century "capitalist revolution" is, like her targets, twofold: first, citing the work of Robert Brenner and others she puts agrarian landowners in the place of the bourgeoisie as revolutionists transforming feudalism into capitalism; second, and most interestingly, she draws on her own specialty to argue for the particularly "capitalist" (that is, individualist) nature of English political discourse since that period. She concludes that, rather than being a peculiar capitalism, modern Britain has been peculiarly capitalist, and its economic, political, and cultural defects should generally be attributed to that uniquely complete capitalism. The "suggestion that capitalism is an unambiguously progressive force" (p. 163) that she sees infiltrating even Left historiography with Anderson and Nairn should therefore be rejected, and the irrational and destructive face of capitalism reaffirmed.

As this summary implies, Wood's book is essentially an argument within British Marxism, and takes for granted much that non-Marxist historians would expect to require sustained argument. Totalizing concepts like feudalism and capitalism (as identifying complete social systems, each with its own specific "laws of motion" [pp. 8, 9, 148]), the determining role of exploitation, and the primacy of material base over cultural superstructure are here assumed rather than demonstrated. Thus, it works best against other

Marxists like Anderson and Nairn, who share its presuppositions. As a critique of "continuity" historians it is less persuasive.

Certainly both Clark and Macfarlane, having couched their own arguments in highly provocative form, are vulnerable. Both have already been taken to task by other scholars for downplaying social conflict and for taking the part for the whole, whether concerning the longevity and universality of "individualist" behavior or aristocratic and high-church hegemony. Yet neither scholar has denied that large changes occurred in English society between the Reformation and the Napoleonic wars, only the claim that these changes derived from a fundamental and thoroughgoing transformation of the social system, a transition from feudalism to capitalism. This position is harder to dislodge, since it is shared by a great many more cautious scholars whose work is not addressed or noted here. And the claim, central to her critique of Anderson and Nairn, that modern England has been a uniquely thorough capitalist society, needs more than the cursory discussion she supplies of developments after 1800.

Nonetheless, it is valuable to have a succinct and clearly written brief for viewing British history in terms of the rise and triumph of capitalism. Wood succeeds in making one rethink the supposed traditionalism of British culture, particularly by pointing out distinctively modern and capitalist aspects of British political discourse since the seventeenth century—not only its oft-noted individualism, but, more suggestively, the comparative absence of the concept of the state, its place filled instead by the concept of civil society. Of course, Macfarlane might well agree and observe that these characteristics, like so much else, did not originate in the seventeenth century.

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ANNE SOMERSET. *Elizabeth I*. New York: Alfred A. Knopf. 1991. Pp. x, 636. \$30.00.

Surely historical scholarship would be greatly impoverished were it not for the contribution of the non-professional as well as the academic historian, and this has long been especially true for the writing of English history and historical biography. Thus, one turns to this latest "serious" biography of Elizabeth I, by Anne Somerset, in hopes of finding a new "standard life": somewhere between the hagiographic and outdated tome of John Neale (1934) on the one hand and the disappointingly shallow, not to say glib, revisionism of Christopher Haigh (1988) on the other; one that would bring up to date the contributions of, among others, Neville Williams (1967), Paul Johnson (1974), Lacey Baldwin Smith (1976), and Carolly Erickson (1983) in between. In short, we require a work that would take into account the best of recent scholarship, much of it revisionist, address issues of

current controversy, and still provide a satisfyingly literate narrative.

Although Somerset's biography is not by any means all of these things, it is at least some of them. Somerset attempts to go back to the major sources, many of them long since published, and to weave anew from the raw materials. Her efforts have much to recommend them, especially to a general audience. Somerset writes engagingly and colorfully, treats the conventional canon of Elizabethan issues in appropriate order, and tells a story well. This is a strongly narrative rather than analytic work, judiciously impartial in its loyalties, and well balanced between interpretive extremes. Somerset's Elizabeth is neither Neale's "Gloriana" nor Haigh's slick and manipulative survivor. She is instead both bright, learned, and shrewd on the one hand, and indecisive, stubborn, and short-tempered on the other. As factors in her reign, Puritans are still a force to be reckoned with and even feared—even more so than the recalcitrant Catholics of whom Haigh has written so effectively—but they are no longer quite the organized conspiratorial element familiar in the writings of Neale and the early Patrick Collinson. And in assessing the role of William Cecil, Somerset departs from Conyers Read's wise and heroic servant, but eschews as well Haigh's manipulative alternative.

Yet if the work somehow produces a picture that is pleasingly and often rightly balanced, it remains wholly conventional in its choice of issues. Diplomacy, foreign affairs, and domestic politics form the main focus, while the efforts of the social and economic historians to broaden our sense of England under Elizabeth are but sparsely evident.

More disturbingly, Somerset's scholarship does not always sustain close scrutiny. There is, for example, a curious predilection to prefer old and sometimes ancient secondary accounts—most obviously in the opening chapters with the use of Patrick Fraser Tytler (1839) on the reigns of Edward and Mary, but throughout the whole volume as well—to work that is more modern, sophisticated, and reliable. This allows the author to glide over the complexities of a goodly number of issues that have recently been raised in articles and scholarly monographs and to concentrate instead on telling a good story with color and flair. Thus, for example, although the treatment of the recoinage of 1560, a small but representative section, remains sufficiently vague to avoid challenge, it relies on Burgon's *Life of Gresham* (1839), an essay by Read (1936), and the revised edition of Feaveryear's classic work (1931), eschewing several important studies of more recent vintage that place the episode in a broader and more sophisticated context.

Equally disturbing are an unacceptably large number of factual infelicities. In the opening sections alone, for example, erroneous dates are given for Edward VI's death, Philip and Mary's marriage, and Pole's reconciliation speech to Parliament: small fail-

ings in themselves, but sufficient to undermine our confidence in what follows.

The resulting volume has an air of having been written in the library of a great country house rather than in the archives or the stacks of a well-stocked university library. Pleasant reading it is—lucid, orderly, engaging, plausible in its perspective—but its over-reliance on printed primary sources and antiquated scholarship does not allow it to clock much mileage for scholarly advance.

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FRANCES HARRIS. *A Passion for Government: The Life of Sarah, Duchess of Marlborough*. New York: Clarendon Press of Oxford University Press. 1991. Pp. viii, 421. \$42.00.

Scholars concerned with various aspects of Augustan England will welcome this well-documented biography regarding one of the most forceful personalities of the era. Frances Harris pays close attention to manuscript sources and writes in the most lapidary prose.

Among the most rewarding segments of the work are those relating to power and influence exercised at court and within ministerial circles. The chapters span Sarah's long life (1660–1744) and the more socially acceptable heterosexual aspects of her sexual politics. Sarah's personality, driven by her lust for power, is well portrayed. Harris's intricate, detailed analysis of the financial aspects of the Churchill family estates provides a trove of useful data for an understanding of aristocratic domestic expenditure and fluctuating family fortunes.

Sarah's early career-relationship with Queen Anne was as something of a cupbearer and partner, but she appears to have grown tired of such roles in her quest for power. In 1727, for example, she had serious troubles in her relations with Robert Walpole, which Harris deftly describes. "Still unable to forget that he had once been her husband's clerk she was ready to bridle at any change in his demeanor to her now that he was prime minister" (p. 266). Harris hesitates to point out that probably the only office that would have truly satisfied Sarah was what she had so long been trained for: queen and ruling monarch.

Some unfortunate aspects of the biography need mention. There is little understanding of the four-party system that dominated William's reign. Indeed, the highly volatile country party does not receive attention; the country party, moreover, was to remain an underlying factor controlling constitutional questions throughout Sarah's life. Another flaw in the book lies in Harris's uncritical use of the utterly inadequate edition of the secretarial correspondence of the latter years of William's reign found in G. P. R. James's *Vernon Correspondence* (1841). Roughly a third of the correspondence was left out for no apparent

reason. A full edition of the documents is available (Dennis Rubini, ed., *The Shrewsbury Papers* [1981]).

Women's history as it should be written includes all relevant aspects of sexuality. An attempt to encompass the diversity of sexuality in elite circles requires some speculation, but Harris employs a great deal of conjecture over such other matters of state as finance and politics. Harris simply does not fully deal with the lesbian aspects of Sarah's sexual politics, except for an attack by Sarah on Anne that Harris submerges in a quotation taken from David Green's biography of Sarah (Sarah: *Duchess of Marlborough* [1967]). Edward Gregg told this story at greater length, displaying far better analytic skills and providing additional quotations left out by Harris at great loss to the readers' understanding of Sarah's personality and tactics (*Queen Anne* [1980]). While Sarah's polemicist Arthur Maynwaring is given a whole chapter, his broadside attack on a second cousin of the Lord Treasurer, Robert Harley, and the influential Abigail Masham, who replaced Sarah in Anne's affection, is not incorporated. Several articles that first appeared in the *Journal of Homosexuality* dealt at length with this aspect of Anne's reign. (See George Rousseau, "Introduction to the Love Letters: Circumstances of Publication, Context and Cultural Commentary," 19 [1990], 47–91, esp. 67–69, and my essay, "Sexuality in Augustan England: Sodomy, Politics, Elite Circles and Society," 16 [1988], 349–81. Also noteworthy is Randolph Trumbach's essay, "London's Sapphists: From Three Sexes to Four Genders in the Making of Modern Culture," in Julia Epstein and C. Straub, eds., *Body Guards* [1991], 112–49.)

Perhaps these problems will be worked out in a second edition of this otherwise fine work. Harris's biography is clearly required reading and likely to remain quasi-definitive for some time to come.

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JOSEPH M. LEVINE. *The Battle of the Books: History and Literature in the Augustan Age*. Ithaca: Cornell University Press. 1991. Pp. xiv, 428. \$29.95.

During the reign of Charles II, a literary controversy surfaced with more acrimony than anyone might have anticipated. In what Jonathan Swift would call "the battle of the books," English advocates of modern scholarship challenged the age-old belief that a gentleman needed only direct knowledge of the ancient historians and moralists in order to live the good life. The moderns argued that the new science of philology had rendered the wisdom of the ancient authors obsolete. Joseph M. Levine has provided us with a learned analysis of this great drama as it unfolded from the Restoration to the time of Alexander Pope's publication of *The Dunciad* in 1729.

Although Levine remains steadfastly neutral in his assessment of the arguments on each side, it is clear

that the champions of the ancients, starting with Sir William Temple, were on the defensive. In the 1690s Temple was outraged by "young barbarous Goths or Vandals breaking or defacing the admirable statues of those ancient heroes of Greece or Rome" (p. 38). The ancients had combined style with content to produce perfect works of instruction, Temple insisted, and their histories should be read for guidance in life because of their eloquence and wisdom. The great modernist William Wotton of Cambridge countered that knowledge of the new linguistic sciences provided a more sophisticated understanding of the ancient world than the ancients themselves had ever obtained. Thus, Wotton expressed the irony of Renaissance humanism: by studying ancient texts with precision, the moderns had surpassed their venerable models. Philology as well as the perspective of time had made the moderns wiser if not more poetic.

By the eighteenth century, the battle lines were drawn between the value of ancient rhetoric and the techniques of modern scholarship. Swift followed Temple's lead in his mock history of the battle, *A Tale of a Tub* (1704), where modern scholarship is subjected to bitter sarcasm. Alexander Pope's translation of the *Iliad* (1715) enraged the modern scholars because of his insistence on the superiority of the older Greeks over the newer Romans, of the shield of Achilles over the shield of Aeneas. Pope responded to his critics in the *Dunciad Variorum* (1729) with a special pungency, exposing the triviality of those who criticized the technical aspects of his translation. The carpers had to be silenced!

The study of history fueled the debate. England did not have a reputable modern history. Temple attempted to provide one based on the ideal of imitation, but his *Introduction to the History of England* (1695) was roundly criticized for its casual use of sources. Wotton's *History of Rome* (1701) was a credit to modern scholarship and the new historiography, yet he was unable to dethrone Livy. Levine concludes that the battle ended in a draw, with the ancients still superior in the narrative and the moderns ahead in philology and antiquities. But the author is not persuasive here: the voice of the ancients was shrill throughout. They were threatened by the advances of modern learning and were unable to incorporate effectively the discoveries of the moderns, whereas the moderns never discredited ancient rhetoric. With the coming of the Enlightenment, and then historicism, it was clear that the future belonged to the pioneers. This is a nourishing study that comes to life in Levine's elegant prose.

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ROBERT B. SHOEMAKER. *Prosecution and Punishment: Petty Crime and the Law in London and Rural Middlesex, 1660–1725*. (Cambridge Studies in Early Modern

British History.) New York: Cambridge University Press. 1991. Pp. xviii, 351. \$64.50.

Unlike most studies of crime in early modern England, Robert B. Shoemaker's book focuses on misdemeanors rather than felonies. The advantage to be derived from such an investigation is that persons accused of misdemeanors were nine times more numerous than those charged with felonies. If the victims, plaintiffs, and informers are also included in such a study, a very broad-based survey of popular attitudes toward the law becomes possible. Regrettably, Shoemaker concentrates on how these persons, together with the Middlesex justices, manipulated legal procedures rather than on broader concepts of law and justice.

We learn, for example, that recognizances could be used not only to guarantee the defendant's appearance in court but also to ensure good behavior without going through the process of indictment and trial. Anyone who could not post bond or find a respectable person to enter into sureties for his or her behavior was likely to be summarily convicted and committed to a house of correction for punishment. Such individuals were typically newcomers, persons of low social status, and frequently female. Many of those swept up and committed to New Bridewell, Clerkenwell, or the Westminster House of Correction in Tothill Fields were victims of the anti-vice campaigns waged by the various societies for the reformation of manners and typically came from previously fashionable but deteriorating West End parishes, such as Covent Garden, that were undergoing rapid change.

Of those who were indicted, most pleaded guilty in order to avoid the time and expense of a trial and were often given low fines. Informal settlements were an important means of arbitrating disputes between plaintiff and defendant without resorting to the expense of formal prosecution. Compensation could be offered to the victim, which was not always possible when prosecution went forward. Such settlements were sometimes marginally illegal, but the parties at issue might be pressured to accept them because they restored or maintained communal harmony and enhanced the prestige of the justice, especially when the latter was courting popularity.

Shoemaker's book is based on a prodigious amount of research systematically conducted and summarized in numerous tables and appendices. He is very good on the procedures and informal settlements employed by individual justices both privately and in petty sessions. But one wishes he had more to say about the nature of the crimes committed and their social and political contexts. Breaches of the peace constituted between 40 and 50 percent of all indictments, apparently including defamation and seditious libel, but, for all his number crunching, the author does not break these offenses down into precise categories. It would be helpful to know some-

thing about the circumstances in which these seditious libels were committed. Were they uttered in coffeehouses or alehouses? Landlords were frequently charged with keeping disorderly houses, and in 1675 Charles II had attempted to suppress coffeehouses. Rarely do we learn about the specific nature and circumstances of such offenses.

The book's title is also misleading. The City of London within the bars as well as the walls is completely excluded; the discussion concentrates on Westminster and the western suburbs. While it is true that these socially diverse and highly urbanized parishes were experiencing rapid demographic expansion and social change and consequently generated most of the crime, one longs to know more about the socially more homogeneous East End suburbs, where a higher proportion of disputes were settled informally and where miscreants were rarely committed to New Bridewell because it was too far away. Again, despite the subtitle, rural Middlesex is dismissed in a few paragraphs. No mention is made of Edmonton Hundred, where poaching remained endemic. We must turn to E. P. Thompson's *Whigs and Hunters* (1975) to learn about deer stealing on Enfield Chase. Illegal venison and game were widely vended throughout Middlesex, but such offenses are neither analyzed nor discussed. East Enders were remarkably successful in resisting the government's attempt to ferret out conventicles, but we learn this only in passing and the topic is never discussed in depth. What discussion there is of the East End and the rural parts of Middlesex comes only in the last chapter and appears to have been added as an afterthought. Shoemaker's book remains a valuable contribution to the study of early modern crime, but one regrets the lost opportunities for a more comprehensive treatment.

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PETER LINEBAUGH. *The London Hanged: Crime and Civil Society in the Eighteenth Century*. New York: Cambridge University Press. 1992. Pp. xxvii, 484. \$44.95.

Peter Linebaugh's earlier studies of the Ordinary of Newgate and the Tyburn riot against the surgeons foreshadowed a larger work on the subject of crime, and this book, the product of many years of research, exceeds expectations. Astonishingly far-reaching, his work touches on the business of Caribbean slave-trading and American tobacco-growing, changes in Irish landholding and Scottish philosophy, as well as the daily tribulations of London tradesmen and provincial bankers. Linebaugh weaves a broad tapestry of eighteenth-century urban life in the archetypal imperial nation.

Linebaugh uses the gallows as a means of tracing a multiplicity of plebeian lives, and from these he pries open a universe of experience. We hear of the skilled



craft of sought-after coopers (among them many Africans) who literally shaped the transatlantic tobacco trade, of defeated meat market sellers turned highwaymen, and of female servants who stole everything from laundry to false teeth. Reminiscent of the work of E. P. Thompson, Linebaugh's strategy is nevertheless uniquely his own. A forthrightness conveys the uncontained energy of the eighteenth century, most of all its many forms of hunger. At all levels of society people focused on material and edible goods. Linebaugh begins at the end, when authorities determined that goods had been misappropriated. He then works backward to the context in which people acted.

Linebaugh questions an ahistorical definition of the criminal. Laws, after all, are written and rewritten by people, and so they reflect changing values and concerns as well as timeless ethical standards. In the eighteenth century, property laws were much in flux. As a result, many ordinary people gathered, pinched, pocketed, or otherwise stole what belonged to others. Although instances of capital punishment were down since the Elizabethan age, the number of capital offenses was rising. Legal historians have long recognized this and developed a literature on the theater of the gallows and the deterrence of the statute book. Linebaugh parts company with these scholars in a more ambitious aim, to highlight the confluence of the law and the capitalist system as it unfolded in its transatlantic setting. One is reminded of Sidney W. Mintz's *Sweetness and Power* (1985) and Eric Williams's *Capitalism and Slavery* (1944; 1966), but with a distinctive twist. What are we to make of the criminal activities of the poor alongside the questionable ethics of larger commercial ventures that depended on low-wage or unfree labor and a certain amount of officially condoned subterfuge?

Linebaugh has gone one step further to uncover the shrewd comprehension of the common people. Not only did they know what was what in the new age of capitalist consumption but they also consciously moved within and across the boundaries of custom and law. Linebaugh convincingly shows how an absolute notion of private property did not exist within the world of eighteenth-century production. Such indeterminateness was not confined to rural land, the context in which Marx identified the problem. A waste economy thrived in many trades and manufactures. Laborers might consume the "waste" materials themselves, or fashion them into items for sale. These were the gleanings of nonagrarian life, the appropriations that came under the various categories of "customs, latitudes or cheats" (p. 264). At the beginning of the century, Bernard Mandeville and his contemporaries recognized their ubiquity; by the end, various statutes against pilfering and embezzlement outlawed them.

Some might argue that the system had to change in order to succeed (and profit), but at the time, the *menu peuple* offered dozens of reasons why their

actions were legitimate—the simplest being that most were living hand-to-mouth. Linebaugh's sensitivity to the needs of common workers enables him to make a strong case for customary rights in the face of modernizing restrictions. Wage rates, family budgets, and especially the conditions of apprenticeship (a system that was "in crisis" [p. 101] in the eighteenth century) suggest that the average worker needed additional sources of income. The more daring explanation, and one that Linebaugh would like us to consider, is that such acts occurred because the system distributed wealth inadequately. Contemporaries, rich and poor, took it as fact. Some of the rich wrote about it; some of the poor acted on it. We cannot know for sure if some crimes were compensatory or vengeful, but Linebaugh's treatment invites this consideration.

The technical achievement of Linebaugh's book is its emotive and associative power. Linebaugh skillfully intertwines narratives of poverty with economic and philosophic analyses, forcing us out of easy categorizations. He takes us through the trades of watchmaking, shoemaking, hatmaking, and tailoring, in effect, clothing the eighteenth-century body and creating a sense of how desire and need melded into one in the world of eighteenth-century consumption. We are not shocked by a tailor's theft of "three bed-sheets and fifteen napkins," but we recoil at the fact of his hanging for the offense (p. 244).

Linebaugh seeks to revise Michel Foucault's *Discipline and Punish* (1977). Rather than depicting the "incarceration" of the eighteenth-century poor, Linebaugh argues for their "excarceration." The persistence of customary appropriations and popular challenges to authority such as the "delivery of Newgate" during the Gordon Riots show the common people not simply as passive victims, but acting on their own behalf against the constraints of the evolving judicial system. Linebaugh claims that power is reasserted through a different means, the wage, which eclipses all other forms of payment. Because of its measurability, the leverage that it provided to employers, and, not least, because it closed the door on workers' claims to customary appropriations, the wage became the central carrot and stick of industrial society. Here Linebaugh's argument would have benefited from further analysis of the women in his evidence. As major pilferers and timber-gatherers, they were marginalized by crafts and trades, not to mention wage rates. The concept of the breadwinner's wage was gendered as male, leaving the subsistence work of women without any formal recognition. Herbert Heaton long ago argued that women predominated in prosecutions against embezzlement in the Yorkshire woollen industry, most likely because they were unorganized. Linebaugh could have made more of this salient division within the working class, which exposed the law in a less than favorable light.

This is a magisterial book. Few recent works reflect as global an understanding of the eighteenth century and its people, and its unforgettable re-creations of



London topography and colloquial language will help to make it a classic.

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MARC BAER. *Theatre and Disorder in Late Georgian London*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xii, 291. \$69.00.

Marc Baer's rich and multifaceted case study suggests new perspectives on Georgian theater, society, and political culture, and the ways in which they interacted. In 1809 the Old Price (OP) riots disrupted performances at Covent Garden Theatre for sixty-seven nights. Destroyed by fire in 1808, the theater had cost a fortune to rebuild. When it reopened, the manager, famous actor John Kemble, had raised prices in the pit and displaced other seats to make room for private boxes. OP rioters resisted these changes, wearing OP hats, carrying OP banners and broadsides, and performing shenanigans in the pit: races, mock sword-play, wrestling, and, above all, the OP dance. The magistrates ordered a few arrests but did not attempt to suppress the disturbances. The courts sided with the OPs. The management ultimately conceded to OP demands.

Baer has done exhaustive research among manuscripts, pamphlets, the popular press, and theater collections. His copious references masterfully knit his story into the fabric of theater, history and theory, anthropology, and social, cultural, and political history. Four themes are especially significant. First, he demonstrates that the OP crowds acted out a variant of E. P. Thompson's moral economy, resisting new prices not only as innovations but also as a unilateral imposition on an audience that demanded negotiation. The private boxes portended social exclusion and sexual immorality. The licensed patent theater was a public trust, not simply private property. Second, Baer insists that Georgian culture at many levels was theatrical: all ranks were keenly interested in the stage. Social relations, acted out in patricians' ceremonies or plebeians' riots, were often theatrical, ritualistic, and hyperbolic, containing elements of performance and melodrama, and also bounded so that, like melodrama, they might "exude violence without being violent" (p. 63). Third, the theater was part of the popular constitution, a kind of cultural church wherein national character and values like freedom, fairness, and spiritual egalitarianism were celebrated in a kind of secular worship: ritualist, participatory, edifying and uplifting, and cathartic. Traditionally theater audiences were themselves actors, active critics used to rendering noisy judgments on performances, sometimes even calling for favorite turns and dismissing tedious ones. Populist empowerment and moral economy drew in the Westminster radicals to form a "steering committee" to negotiate for, though not to direct, the OP rioters. Fourth, all of this

contributed to England's political stability. Baer's discussion is somewhat elusive, but he seems to mean that theater and traditionalism not only bounded plebeian aspirations but also relieved and diffused social and political resentments, whether by catharsis, satire of aristocratic pretensions, or audience enfranchisement. Perhaps the OP rioters, like other plebs, chose to defend a world in which they had some sway, rather than to demand new and uncertain arrangements, while authorities also preferred concession to the costs of repression. Baer also shows how the customary political culture defended by the OPs was being eroded as much by cultural as by social and economic change.

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H. V. BOWEN. *Revenue and Reform: The Indian Problem in British Politics, 1757-1773*. New York: Cambridge University Press. 1991. Pp. xi, 204. \$47.50.

Building on the foundations laid over forty years ago by Lucy Sutherland (*The East India Company in Eighteenth-Century Politics* [1952]), this incisive monograph reconstructs the first critical phase in the relationship between the East India Company and the British state. For Sutherland, the period between the Seven Years' War and Lord North's Regulating Act of 1773 was primarily an episode in the intricate continuum of company and Westminster politics. H. V. Bowen, by contrast, extends his brief to the problems of the British in the subcontinent itself.

The result contributes very effectively to the current reassessment by C. A. Bayly and others of Lewis Namier's "Imperial Problem," which is shifting the composition of the picture somewhat away from the single and hitherto dominant Anglo-American axis in favor of a larger and more complex interplay between several mutually dependent parts. In the process, not only North's reputation as a parliamentarian but also his claim to remembrance as an imaginative and far-sighted statesman and administrator are further enhanced. Far from being a slipshod product of confusion, indolence, and executive patronage memorable only for its indirect contribution to the American debacle, the 1773 Regulating Act, compromise though it was, emerges as an essential measure for the future development of the company's position in India. It was produced by a government acting from strength under an effective and skillful leader, whom Laurence Sullivan at least thought "the boldest minister this realm has been blest with since the days of Oliver Cromwell" (p. 189). The stock of North's opponents suffers by contrast.

Another interesting, and related, result is the insight Bowen offers into the first appearance of ideas and attitudes more usually associated with the 1780s and postwar adjustment. Bowen's concerns remain

political, but in a period when politics and state alike were being confronted with new and increasingly difficult situations, what he has to say on that score deserves the attention of a wider readership as well. If nothing else, he provides a reminder that there is a long and far-reaching history behind the "modern" problem of the international corporation or private-sector consortium which gets drawn by accident or design into situations it cannot fully control that implicate the security of the public credit, the authority of the state, and the stability of the existing world order.

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A. M. C. WATERMAN. *Revolution, Economics and Religion: Christian Political Economy, 1798–1833*. New York: Cambridge University Press. 1991. Pp. xvi, 310. \$49.50.

The first purpose of this book, A. M. C. Waterman says, is to understand the tradition of "Christian political economy" that formed the mainstream of Anglo-Scottish social theory in the first three decades of the nineteenth century. Yet he also comments more modestly that his volume may be regarded as "an extended footnote, or technical appendix to" Boyd Hilton's *The Age of Atonement: The Influence of Evangelicalism on Social and Economic Thought, 1795–1865* (1988), another recent study of the crossbreeding of classical political economy and Anglican theology attempted by Thomas Robert Malthus, William Paley, J. B. Sumner, Edward Copleston, Richard Whatley, and Thomas Chalmers. The second self-description is the more accurate of the two. Well over half of this volume is devoted to a very close examination of the first edition of Malthus's *Essay on Population* (1798) and its subsequent revisions. Within his own professed scholarly subdiscipline—that branch of the history of economic thought associated with the journal *History of Political Economy*—Waterman's lengthy demonstration of the complex historical identity of the *Essay* may represent, as he claims, "a somewhat eccentric" approach (p. 7). But in this regard he has nothing new to offer cultural and intellectual historians familiar with recent scholarship on Malthus and his intellectual milieu.

There can be no question that criticism and development of Malthus's ideas by himself and others were central themes in "Christian political economy." Yet the story hardly begins with Malthus and is considerably more intricate than Waterman manages to suggest. One major flaw here is Waterman's taking for granted that, at the time Malthus's *Essay* first appeared, Adam Smith's *Wealth of Nations* (1776) had already firmly established its reputation as the founding text of what Hilton called "high political economy." Recent scholarship, however, has shown that the tradition Smith is so often said to have founded had

something of an oddity for its source. It never occurred to Smith that he ought to help bring a new tradition into being; we know, for example, that he never recruited any disciples, nor did he ever refer to himself as a "political economist." The phrase "political economy," for that matter, first appeared at the outset of Book IV, not quite halfway through a huge book that Smith stressed had managed to define only one of the branches of the science of legislation.

None of this ever prevented proponents of political economy from later proclaiming the *Wealth of Nations* as the founding text of their new intellectual tradition. But the peculiarities of that text—including what J. R. McCulloch thought was its confused and illogical arrangement—also required Smith's self-styled disciples to remake a model of their founder that they could then follow, and this transformation came more slowly and at a much later date than Waterman recognizes. The historical mapping of *Wealth of Nations* provided by political economists was drawn, at the earliest, some forty years after the book was first published; moreover, in 1816 it appears that there was only one person in Britain—Malthus himself—whose main intellectual energies were devoted to political economy and who even thought of himself primarily as a political economist.

Viewed from this angle, Malthus's relation to Smith in 1798 remains inadequately understood on at least two counts. Founders of a tradition typically leave a definite project for their heirs to work on, but since Smith had not yet been cast as the founder of political economy when Malthus's *Essay* first appeared, it can hardly be said that Malthus set out to render the discipline consistent with Christianity. Indeed, during the early 1790s, Smith had been enlisted as a hero by British sympathizers with the French Revolution; after 1793, he was also frequently cited as an authority by liberal reformers who opposed continuation of the wars with France because they were thought to diminish Britain's economic productivity. These now largely forgotten Jacobin interpretations of the *Wealth of Nations* also account for the second largely unexplained feature of the Smith-Malthus connection in 1798: that Malthus should have seized on Smith in an effort to define a middle ground between revolution and reaction. To the end of the 1790s, outside of a small community of reformers, Smith's liberal economic doctrines remained suspect in Britain, because most thought they required sweeping political change to be accomplished.

Much of what might be called the prehistory of "high political economy," then, remains to be written. Once we come to understand how the pieces of that tradition happened to come together as they did, we will have defined the proper starting point for a more adequate historical understanding of its alliance with Christian belief.

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MARY E. FISSELL. *Patients, Power, and the Poor in Eighteenth-Century Bristol*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1991. Pp. xi, 266. \$54.50.

The eighteenth century has come to play a prominent role in the social history of medicine. Historians have discovered in these years an ebullient medical consumerism, uncovered the tender roots of professional medicine, and located the source of a widespread medicalization of society. Here, too, according to Michel Foucault, the body was first objectified and silenced. Patients' "voices" faded into near inaudibility as medicine became increasingly hospital-oriented and dominated by clinical pathology. Instead of listening, physicians now peered into silent corporal caverns in their search for bodily truths, thus uncoupling the body's owner from his or her own perceptions of health and illness. Mary E. Fissell's book follows all these themes, linking them to the shifting class relationships of eighteenth-century England.

This book is largely about the Bristol Infirmary. Yet if Fissell concentrates on the hospital, her analysis does not remain locked away within the Infirmary's walls. She addresses the entire early modern medical system (or rather systems), arguing that "strong similarities in medical and lay ideas and practices" persisted and that, therefore, "large numbers of early modern people held in common many ideas about the body" (pp. 16–17). According to Fissell, this substratum of common knowledge and common practice "mitigated against the creation of professional autonomy and power within a doctor-patient encounter" (p. 29). In a world of luxuriant medical pluralism, almost everyone to some extent acted as "their own physician." Yet they also consulted a plethora of healers and chose from an overflowing cornucopia of therapies. Moreover, in this environment, unlike in our own "medicalized" one, it was the body's surface—visible to all, readable by everyone—not the body's interior—opaque, susceptible only to expert interpretation—that betokened disease. This physical openness, too, helped prevent the arrogation of medical authority by any group of practitioners.

The question that drives Fissell's investigation is how did we move from a premodern system of pluralism and patient volition to a modern pattern of passive patients and medical authoritarians? Fissell attributes the source of that change to the "process by which inequalities of rich and poor became those of nascent class relationships" (p. 200). The hospital first showed the effects of these medical and social metamorphoses. There is, of course, little original about locating eighteenth-century medical transformations in the hospital's areal. Foucault did it, and so have many others. But Fissell's focus differs significantly from Foucault's and from that of many scholars who have written on the rise of hospitals and clinical medicine: she centers her story on the patient's experience.

As the governors of the Infirmary, mostly Dissenters, retreated from the day-to-day running of the hospital, the staff surgeons were simultaneously converting the hospital into a "training ground" in which patients served as "clinical material, both before and after death" (p. 199). Thus, the poor—the principal inmates of the Bristol Infirmary and of most eighteenth-century hospitals—slowly lost control of their own bodies, forfeiting the validity of their own concepts of health, illness, and physicality. Fissell couples the rise of dissection, and of the resurrection men who flaunted laws and offended propriety in their pursuit of cadavers and body parts, with these alterations in hospital practices, with the rise of the surgeon's dominance within the Infirmary, and with the shaping of an embryonic surgical "profession." The Bristol Infirmary, in Fissell's evocative prose, not only stole "the bodies of those who died in hospital, it also denied meaning to those who lived" (p. 200).

This brief review cannot do justice to the way in which Fissell skillfully draws together several analytical threads. Her discussions of the waxing power of surgeons, the social role of the Infirmary, and the corporal disenfranchisement of the poor are based on meticulous research and reflect an enviable command of the secondary literature. This produces a colorful tapestry of interpretations and meanings that reveals how the older fabric of medical pluralism unraveled. And yet one wonders if part of the story is not still missing. For example, would it not be just as feasible to argue that there was also at work here a medicalization "from below," which was a "more dynamic and dialectical process involving changing patterns of demand" and not merely "supply-side" suppression, as Colin Jones has suggested in another context ("Montpellier Medical Students and the Medicalisation of Eighteenth-Century France," in Roy Porter and Andrew Wear, eds., *Problems and Methods in the History of Medicine* [1987], 58). To what extent did people voluntarily give up control to experts, rather than having their corporal identities ripped from them by striving proto-professionals? Both interpretations are plausible, but raising these other possibilities does nothing to detract from the value of this fine monograph.

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NEIL J. SMELSER. *Social Paralysis and Social Change: British Working-Class Education in the Nineteenth Century*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xii, 499. \$35.00.

Neil Smelser began his scholarly career with a sociological study published as *Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry* (1959). Thus, according to Smelser, the present book marks "a return to my youth" (p. xi). During the third of a century that elapsed between

the two books, Smelser wrote prolifically on a wide range of sociological topics and became one of the most eminent sociologists in the United States. That he avoided major forays into British social history for so long is understandable. Although *Social Change* made a big splash when it was published, the critical tide ultimately turned against it. The works of Michael Anderson, M. M. Edwards, and Roger Lloyd-Jones have all but vitiated its main conclusions.

The present book is not Smelser's answer to such critics nor is it a defense of *Social Change*. Duly chastened, it seems, Smelser takes two new tacks. His theoretical observations are, for the most part, better integrated into the text than in *Social Change*. There is no filling of "empty boxes" here. Furthermore, he moves a few steps away from the equilibrium model that provided the theoretical centerpiece of the earlier book. Now he focuses on "equilibrium-in-tension" and states that his account "is intended to yield a picture of a balance of forces, but this balance is not static in any way, since the various forces may stand in tension or conflict with one another" (p. 23). This is a conceptual shift that will probably not cause many historians to sit up and take notice.

Smelser's main achievement is to offer a comparative survey of educational policy and practices in the four main regions of the British Isles from 1820 to 1870 (with some backward glances). For most readers, his accounts of educational developments in Scotland, Wales, and Ireland will be the most novel parts of the book. In each case, Smelser draws together a great many statistics from official sources and provides case studies of episodes in which clashes over educational policy became particularly contentious. For Ireland, he especially emphasizes the fate of the combined system (enacted in 1831) in which common secular education was combined with separate Catholic or Protestant instruction. His digression on simultaneous Catholic-Protestant squabbles over education in New York City (pp. 218–31) gives added resonance to this discussion.

His account of English education occupies most of the book and offers little that is surprising either factually or theoretically. Among other things, the background to the major education acts (for example, those of 1833 and 1870) is rehearsed in considerable detail, complete with summaries of the windy parliamentary debates. One of Smelser's most interesting chapters traces the emergence of such new occupations as the educational inspector and the pupil-teacher. Yet many of these discussions have a textbook quality, complete with lists of main points in lieu of narrative or argument. Although not strewn with jargon, the book contains a number of quirky (or trendy) usages that are included, perhaps, to add touches of drama to this oft-told tale. Virtually anything having to do with religion is labeled "primordial"; thus, increasing religious tensions in the 1840s furnish evidence of "primordial proliferation." New educational policies often signify "truce points," while

the absence of significant policy changes becomes "paralysis."

Smelser states that he has read all the parliamentary debates and papers on educational issues for the period from 1807 to 1880—a herculean task that may make him unique among writers on nineteenth-century British education. Yet immersion in official printed sources betrays a tacit assumption. It is that a balanced understanding of British educational policy and practice does not require an examination of any of the relevant manuscript sources, any of the major newspapers, or more than one contemporary periodical (the *Transactions of the National Association for the Promotion of Social Science*). This assumption is highly debatable, to say the least. There are limitations, moreover, in the secondary works he employs. The journal and monographic literature available when he began this project in the late 1970s is cited extensively. Indeed, this book provides a convenient guide to the state of educational researches at that time. Smelser, however, demonstrates only a minimal awareness of more recent work. When he moves beyond purely educational topics, the situation becomes even less satisfactory. For Victorian society, he relies on such writers as Kitson Clark and W. L. Burn; for the economy he turns to W. H. B. Court and John Clapham.

In sum, this book largely fails in one of its major objectives, that is, in breaking new ground in the sociology of public policy formulation. But it succeeds in something more modest. As a comparative synthesis of education in England, Wales, Scotland, and Ireland, it furnishes a useful (though hardly definitive) contribution to the burgeoning "Four Nations" literature.

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SONYA O. ROSE. *Limited Livelihoods: Gender and Class in Nineteenth-Century England*. (Studies in the History of Society and Culture, number 13.) Berkeley and Los Angeles: University of California Press. 1992. Pp. xi, 292. \$39.95.

Some of the best labor historians of the past twenty years have mined the rich archival sources of provincial England to study the vexed and complicated relationships of "masters and men" during the first Industrial Revolution. Sonya O. Rose's book adds gender to this formulation and shows how changing attitudes about what constituted a good woman and a good man among the working class, managers and owners, and the general public shaped the workplace. As such, it is both an important summary of scholarship on capitalism and the family and a major contribution to it.

Rose contends that, at all levels and in all work relations, gender was fundamental to the ways work was defined, labor disputes negotiated, and strikes



defended. The Ten Hours Movement of the 1840s, as part of its campaign to limit the hours of children and women in textile factories, argued that the family wage, in which a man earned enough to support a family, was a fundamental right. It was a new right, characteristic of new working conditions that separated the workplace and the home. A man's ability to sell his labor on the open market came to represent his masculinity; work defined a man's public identity as a rational, responsible adult. Simultaneously, domesticity—and especially an idealized motherhood—came to delimit a woman's identity, so that by the 1870s married working women were blamed for infant mortality and their husbands for living off ill-gotten earnings that took jobs from other men. Even single women were bound by their domestic potential rather than by their work skills.

Rose's most telling evidence in this richly documented book is drawn from that most familiar of sources, the Lancashire cotton industry. Here women and men worked side by side at power looms and were paid the same rates. And yet numerous distinctions existed and were exacerbated in times of stress. The unions never successfully negotiated a set rate; piecework inevitably favored men. Male weavers routinely cleaned and adjusted their looms after working hours, when the women could not legally continue working. Only men could become overlookers; Rose has found several examples of sexual harassment, with the collusion of the management, by overlookers. During a series of strikes and lockouts in 1878, the all-male union leaders employed the rhetoric of the family wage, calling on the striking workers—men and women—to fight for a higher wage that would enable the men to support their families and wives at home. Women workers were repeatedly discouraged from speaking in public or becoming active union participants. After a stone-throwing incident, the *Blackburn Standard* contrasted the behavior of “a collection of half-dressed Irishwomen” with the sober manliness of native workers (p. 170).

Rose, however, does more than simply add gender to a familiar picture of job scarcity and labor unrest. She also documents the gendering of legislation and public policy. The Poor Law Amendment Act of 1834 created the fiction that the state would not interfere with economic relations and was a neutral observer between capital and labor; working men were responsible for their own—and their families'—impoverishment. A man whose wife had demanded poor relief was responsible for repaying her costs, whether they lived together or not. If a man entered the poorhouse his family was forced to follow, whether or not the wife could earn a living.

A series of factory acts over the ensuing decades regulated only those defined as “not free agents,” that is, women and children who worked in specified industries outside the home. Women workers came to be seen as irresponsible mothers and dupes of their husbands, a position that made it easier to pass

protective legislation, but that insured job segregation and the continued inability of women to compete for well-paid employment. In effect, men were trapped into blaming themselves if they were unable to support their families during periods of economic depression, and women who worked, whatever their circumstances, were unnatural mothers. At the same time many manufacturers evaded legislation through outwork; sweated home industries grew, further lowering wages of the unorganized.

Some of Rose's most interesting comments concern the construction of working-class masculinity around physical labor, technical skills, and workplace loyalties—and, as the century progressed, earning a family wage. I would have liked more discussion of how these attributes of masculinity were constructed and sustained in spite of the fact that large numbers of men faced deskilling and fierce job competition and that fully 80 percent of working-class families needed supplemental income. The inevitable tensions of this situation are only briefly mentioned in reference to the possible growth in domestic violence. Why did so many working men hold to an economically impossible ideal? The bitter union campaigns—often with public support—to deny women access to skilled jobs, however, has seldom been better analyzed.

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JUTTA SCHWARZKOPF. *Women in the Chartist Movement*. New York: St. Martin's. 1991. Pp. viii, 337. \$49.95.

The cultivation of women's history has yielded a rich crop and, while the harvesting sometimes seems more like gleanings, the field is never barren. Jutta Schwarzkopf is not the first historian to study women in the Chartist movement and the data are not plentiful, but she has an original approach: “It is the impact of Chartism on the construction of gender and on gender relations as an integral part of the formation of the working class” (p. 4). The book covers the changes in plebeian women's living conditions brought about by industrialization, the Chartists' utopia, the social profile of the female following, the women's view of their role both in the family and in public, gender relations within the movement, post-Chartist activities, gender and class in Chartism, and, lastly, Chartism's transitional character in women's political history.

Schwarzkopf makes extensive use of novels (by three male Chartists) as well as speeches and newspapers, and presents overwhelming evidence that patriarchal attitudes were not imposed by the middle class but were indigenous. Capitalism and the factory system had weakened male hegemony, and through Chartism working-class men strove to reestablish the patriarchy. “The Chartist idea of gender relations became the movement's legacy to subsequent gener-



ations of workers" (p. 284). The logic is not altogether convincing; can one be left what one already has? Here, as throughout, the intrusiveness of Schwarzkopf's ideology leaves some doubt about the reliability of her interpretation in the mind of one who is not of her persuasion.

Differences in ideologies (and senses of humor) can lead to different reactions to the evidence; space allows only one example of many. Of the condemned Chartists, Schwarzkopf writes: "All [they] could do was to find someone to stand in for them in their capacity as the protectors of and the providers for their families. And so they committed their unprotected dependents to God's mercy" (p. 137). Differences exist even to what constitutes evidence: "The lack of any evidence of this happening does not rule out the possibility" (p. 214). That is true, and it opens up a truly awesome range of historical subtexts.

The concentration on gender attitudes makes for an enlightening if depressing read. So many of the Chartist leaders had clay feet and muddled minds from the feminist point of view. More depressing, though, is that Schwarzkopf's focus results in a picture of the women either starving in their patriarchal homes or working long hours in the factory, or both—statistics show otherwise—and the 12,000 women who found time to attend an all-female rally get short shrift. (Both Thomas Salt and Henry Vincent are credited with being the only man to convene all-female meetings [pp. 210, 224].) Much more attention is given to the Female Chartist Associations, but even in their heyday (as opposed to "periods of lull" [pp. 181, 186]) they, for the most part, kept their eyes and voices down except in support of their menfolk. It is a good thing for the women's movement that there were some middle-class activists just around the historical corner.

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JILL LIDDINGTON. *The Road to Greenham Common: Feminism and Anti-Militarism in Britain since 1820*. (Syracuse Studies on Peace and Conflict Resolution.) Syracuse, N.Y.: Syracuse University Press. 1991. Pp. xi, 349. Cloth \$32.95, paper \$14.95.

The road to Greenham Common, according to Jill Liddington's ambitious but uneven history of women's antimilitarism in Britain between 1820 and 1989, followed a long, winding, and bumpy route, conveying its late-twentieth-century pilgrims to the American military base in Berkshire, where cruise missiles were to be installed, by way of the Crimea, South Africa, northern France, and Hiroshima. Along the way, women critical of Britain's participation in the Crimean War, the Boer War, world wars I and II, and the Cold War formed associations of varying size, energy, effectiveness, and tenure to protest the fomenting of war and the hardships it created, partic-

ularly for women. Thus, Emily Hobhouse journeyed to South Africa in 1900 to bring relief to the Boer women and children interned in British concentration camps. The Women's International League, formed during the first year of the Great War, held a Women's Peace Congress at the Hague, attracting women from America and all the belligerent countries, who came together to demonstrate their solidarity on behalf of justice and the peaceful resolution of conflict. In the 1920s, the renamed Women's International League for Peace and Freedom condemned British atrocities in Ireland, violent revolution in Europe, and anti-Semitic pogroms in Ukraine, and campaigned for disarmament. Thrown off course by the advent of Nazism in the 1930s, women's activity on behalf of peace reemerged in the 1950s and 1960s within the Campaign for Nuclear Disarmament. With detente in the 1970s, the peace movement lost its urgency, but Ronald Reagan's showdown with the Soviet Union, and Margaret Thatcher's agreement to station U.S. nuclear missiles on British soil, revitalized it. In 1981, thirty-six women marched from Cardiff, Wales, to Greenham Common, setting up camp outside the American military base in hopes of warding off the installation of the missiles. They failed to do so, but hunkered down and stayed for more than seven years. In 1982, after 30,000 women "joined hands in the world's most powerful protest against nuclear war" (p. 1), as the *Daily Mirror* reported, "embracing" the base, the women's camp at Greenham Common became a vivid and enduring symbol of nuclear protest.

This is a dramatic story and Liddington tells it well, if uncritically. Her admittedly "celebratory" account of women's campaigns for peace claims also to offer a critical appraisal of the ideas that connect feminism and antimilitarism. Although she presents the ideas and rationale feminists put forth as they argued the connections between women and peace, she stops short of analyzing them. Her own unproblematic, untheorized linking of feminism and antimilitarism too often slips into a conflation of feminism, however variously constituted, with antimilitarism, which in turn leads to some circular reasoning and perhaps accounts for the lack of a thesis. Historians must do more than simply assert, as Liddington does, rightly, that histories of peace movements that leave women out or fail to analyze the linguistic associations of women and peace are inadequate and misleading, as are histories of feminism that ignore its links with pacifism. We must analyze these links and the historical developments that helped to construct them in the first place.

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JAMES A. JAFFE. *The Struggle for Market Power: Industrial Relations in the British Coal Industry, 1800–1840*.

New York: Cambridge University Press. 1991. Pp. xi, 228. \$49.50.

This book concentrates on industrial relations in what was then Britain's most important coal field, Durham and Northumberland. James A. Jaffe shows that control of the production process was almost entirely in the hands of the men. Had colliery viewers (that is, managers) been less conservative, they could have changed from bord and pillar (where each man works in his own "room") to the longwall system (where men work cooperatively on a long coal face, allowing division of labor), which would have allowed them to exercise greater control. It was not until mid-century, however, that the technological superiority of longwall working came to be recognized by even the younger viewers like T. J. Taylor, and by this time a whole socioeconomic system had been built around bord and pillar.

Since management was unable to exercise control at the point of production, industrial conflict manifested itself principally as a struggle for market power. Proprietors and managers sought to control the main product market through a cartel (the *Vend*). Control of the labor market was exercised through the annual hiring process (the yearly bond) and through the provision of tied houses, from which men could be evicted at short notice.

The early trade unions also sought to control product and labor markets by, for example, instructing members to limit their daily output (rather like the Scottish miners' *darg*) and by vetting applicants for employment. They also fought to defend the practice of workplace bargaining, the importance of which is very well brought out by the author.

Jaffe deals fairly with the union led by Tommy Hepburn and the disputes of 1831 and 1832 in which it was involved. He gives due credit to Robert Colls's account in *Pitmen of the Northern Coalfield: Work, Culture, and Protest 1790-1850* (1987), which he rightly prefers (see p. 179) to the unsatisfactory account in Roy Church's "official history" (*The History of the British Coal Industry*, vol. 3, 1830-1913 [1986]).

In this period, the coal field was dominated by the landed gentry and aristocracy, whose increasingly paternalistic practices were designed to produce a stable labor force. Some proprietors expressed pride in their workmen's houses and the services that went with them. They also recognized the importance of education, religion, and benefit societies, giving both moral and material support to the institutions concerned. The Methodist Societies (and especially the Primitive Methodists) provided the men with most of their union leaders.

In an epilogue the author deals with the collapse of Hepburn's union. Briefly, although employers recognized that their workmen's restriction of output helped them to optimize profits, they resented the union's control of product and labor markets and

were determined to crush the union. As Jaffe says, "Given the choice between profit and power, the coal owners' actions unequivocally proclaim that their social power and its corollary, the control of the market, was more important than their immediate economic interests" (p. 182). By locking out and evicting from their homes thousands of union members, and by restricting the earnings of those in work, they placed an intolerable strain on the union's finances. Some also used the "document" (where employment was conditional on abjuring trade unionism) and recruited "blackleg labour" on a large scale. Because the union became powerless it lost membership rapidly and quickly collapsed. Jaffe follows this account with a brief commentary on union developments in the second half of the century.

Despite some minor imperfections, I recommend this book to anyone interested in the industrial relations of the British coal mining industry.

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EDWIN LEE. *The British as Rulers: Governing Multiracial Singapore, 1867-1914*. Kent Ridge, Singapore: Singapore University Press. 1991. Pp. xvii, 314. Cloth \$42.00, paper \$32.00.

Edwin Lee presents here rather a strict constructionist view of the British as rulers. In examining the period between 1867 (when Singapore passed from the control of the India Office to the Colonial Office) and 1914, he has depended heavily on official sources, both of the Colonial Office and of the government of Singapore itself. Hence we get a picture of society seen through the eyes of British administrators, and the Singaporean response to British rule is presented largely as the British themselves perceived it. What is missing is any suggestion that the reality may have been different, and it would have been helpful to have been given something of a view from below as a check against which to measure the British record.

Although Singapore may be a multiracial society, Lee's main thrust has to do, not surprisingly, with governance of the Chinese. Not only did they make up the largest segment of Singapore society (half of its population in 1840, almost three-quarters by 1900) but they also posed problems for British administrators that were less evident among "the easily governed races" (as Lee calls a chapter dealing with Indians, Arabs, and the Malays). After briefly describing the ways in which the early British colonists neutralized the political powers of the Malay rajas, he moves into his main theme: the problems presented by the Chinese. Of these there were several: tensions between regional and dialect groups (Hakka, Hokkien, and Cantonese, for example), between clans, and particularly the problems presented by the secret

societies, or "hoeys" (to the British, says Lee, the term *hui*, or association, meant a secret society), with their occasional resorts to violence.

A central theme of Lee's book is that British conceptions of liberalism and individualism prevented them from resorting to a system of ruling the Chinese through their own institutions, as in the French or Dutch colonies. Chinese should be the beneficiaries of British law and British institutions, whatever might be the cultural and political difficulties of such an approach. Although Lee admires the policy, his work shows how the British themselves compromised it, seeking the assistance of the Chinese elite, and from time to time actually cooperating with the societies themselves. Eventually, in instituting the office of Protector of the Chinese in 1877, they went further, and W. S. Pickering, the first Protector, ruled the Chinese like a benevolent Confucian mandarin, often meting out justice first and seeking legal sanction for his policies later. And while he originally sought to outlaw the hoeys, he later concluded that they were the only means by which the Chinese, with their very different political traditions, could be governed (although under his successors the hoeys were eventually banned).

The real challenge to British authority, of course, came when a reforming Qing dynasty, and the republic that followed it, began a contest for the wealth, talent, and loyalties of the Nanyang Chinese. Lee only begins this part of the story, but even by 1914 it was clear that the British faced a new problem in competing—not entirely effectively—for the allegiance of at least those ethnic Chinese who were Straits born and were British subjects. Lee uses no standard romanization of Chinese, apparently simply adopting whatever form appears in the British documents he uses. A character index would have been helpful (on page 190 two Chinese characters do appear—the only ones in the whole book); or if not that, at least a transcription of names and terms into pinyin or Wade-Giles.

In sum, this is a useful book for the light it sheds on official British perceptions of the task colonial administrators faced in Singapore.

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HARTMUT BERGHOFF. *Englische Unternehmer, 1870–1914: Eine Kollektivbiographie führender Wirtschaftsbürger in Birmingham, Bristol und Manchester*. (Bürger-tum: Beiträge zur europäischen Gesellschaftsgeschichte, number 2.) Göttingen: Vandenhoeck und Ruprecht. 1991. Pp. 387. DM 96.

This nicely produced study, the second in Vandenhoeck and Ruprecht's projected series on the European bourgeoisie, is an extended analysis of a sample of 1,328 late-Victorian provincial English entrepreneurs. Although the work began as a doctoral dissertation presented at the German university of Biele-

feld, Hartmut Berghoff's *Doktorvater* was Sydney Pollard, and this book shows an admirable grasp of the Victorian social and cultural milieu as well as of the lively ongoing debate of the last decade on the subject of the "failure" of the English economy and the supposed "gentrification" of its business class.

This is, nevertheless, a German dissertation, showing its provenance in its form. Before getting down to his own analysis of the entrepreneur, Berghoff lays out for us "the theory of the entrepreneur" in a rapid sweep from the eighteenth century to the present. The study that follows, however, is not an attempt to substantiate a theory of the entrepreneur with econometrics. It is a social history, built on prosopographical statistics, offering a description of the culture of the businessman rather than an analysis of his economic behavior.

Although Berghoff complains of the neglect of businessmen in elite studies, in fact his work is one wave in a minor flood of books and articles on the Victorian entrepreneur. Martin Wiener's well-known, provocative essay on the "decline of the industrial spirit" in Britain (*English Culture and the Decline of the Industrial Spirit 1850–1980*) and W. D. Rubinstein's detailed analysis of the super-rich, *Men of Property*, were both published in 1981. Since then the debate has surged around the effectiveness of "gentlemen capitalists" in an increasingly competitive international environment; their propensity toward "social expenditure," especially in land; and how the various sectors of wealth—industry, commerce, and banking—related to each other, to the Establishment, and to the economic progress (or lack thereof) of the nation. By the early 1990s, the storm has died down and a consensus of sorts has asserted itself. It does not demean Berghoff's careful and informative study to say that it is illustrative of this post-Wiener consensus that, although the culture of the businessman must surely matter in his economic decision making, the gentrification thesis has been vastly overstated.

Berghoff provides, in a closely written text of nearly three hundred pages, an extended social analysis of a substantial sample of businessmen who flourished in three important but very different centers of Victorian enterprise: Manchester, where cotton was king; Birmingham, with its smaller, more diverse industries; and Bristol, a long-established trading port in danger of decline. His categories for analysis include industrialists, financiers, and merchants, while information about year of birth and probated wills allows analysis by age and wealth as well as region and economic sector. A sample of service professionals (lawyers, doctors, accountants, and the like) is provided for comparative purposes.

These 1,328 businessmen represent, no doubt, a substantial proportion of the successful and relatively well-off entrepreneurial class in these three cities, although whether they are, as Berghoff claims, "typical" of the small and medium-sized enterprises

(p. 12), is more problematic. He restricted his analysis to those who left £50,000 or more at death (£40,000 for the Bristol businessmen) and who could be identified in a variety of local and national sources. This excludes a large number of the modestly successful (until the twentieth century, £50,000 was a quite substantial legacy). We are dealing here with an elite well beyond the local trader, minor stockjobber, or small-shop manufacturer. Such prosopography is inevitably skewed toward the successful who die rich and are memorialized in the panegyric of the provincial press.

Be this as it may, there is a rich vein here that Berghoff mines in a thorough way. In his analysis of social origins, religion, education, marriage patterns, as well as honors and public office, he engages the gentrification thesis insistently and by and large tellingly. A short review such as this cannot do justice to this material, but in general Berghoff concludes that there is little evidence of a cultural capitulation by provincial businessmen to the traditional elite, that only a small minority in any event can be said to have been successful at achieving a national rather than local orientation, and that there does not seem to be any convincing evidence for a successor generation of unmotivated, gentrified sons. If, in all this, he rarely breaks free from the familiar lines of previous analysis, his evidence will provide further corroboration for the anti-gentrification case and may point to areas for profitable future research. Tantalizing references to German business history suggest one such area. One would like to have a deeper comparison of English gentrification and the "Buddenbrook-Syndrom" across the North Sea.

Berghoff's statistical method seems sound, although there are some areas where questions might be raised. Most importantly, he does not make as much as he should of the fact that he is employing partial data, that the "unknown" category is both sizable and variable in many of his calculations. Readers are often left to calculate for themselves just how large a proportion of the sample has been excluded through lack of information pertinent to the question at hand. In some cases, this lack itself may have significance. It would also have been useful to have had a list of the names of the 1,328 businessmen—something that, arranged by region and sector, could easily have been included in an appendix. Finally, much of the generalization which Berghoff extracts from his figures begs for in-depth case studies, as misleading as these can be. One wishes he had been able to pause more often to give his statistics this kind of confirming exploration. Nevertheless, this is an interesting study that belies any prejudice one may have about German scholarship on British social and economic history. It deserves to be made available in English.

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JOHN TURNER. *British Politics and the Great War: Coalition and Conflict 1915–1918*. New Haven: Yale University Press. 1992. Pp. xi, 511. \$40.00.

The course of British domestic politics during World War I has hardly lacked its historians, and from Lord Beaverbrook onward it seems that nearly as much ink has been spilled on the subject as blood. Now with this impressively weighty synthesis John Turner enters the fray. The novelty of Turner's approach lies in his effort to comprehend the transformation of British party politics, broadly defined. Accordingly, he combines a detailed narrative of the high politics of cabinet-level intrigue (including the recourse to coalition government and the eventual destruction of the nation's last Liberal government) with two substantial analytical sections exploring the significance of both the corporatist initiatives in wartime industrial relations and the changes in the postwar electorate wrought by the franchise extension of 1918.

The story he tells is familiar, but Turner's particular interpretive emphases are important, stimulating, and often persuasive. On the much-debated issue of the wartime fracture of the Liberal Party, he resists attributing the split either to the contrast between an indolent Lord Asquith and a dynamic David Lloyd George or to an irreconcilable conflict between a more orthodox Liberal elevation of voluntarist principles and a surge of collectivist sentiment. Philosophical distinctions, the author implies, merely obscured a fundamental dispute over strategy, whereby Asquith sought to husband the nation's finite resources and minimize the resultant social and economic disruption by means of a longer struggle, while Lloyd George was inclined to gamble on a knockout blow to secure a quick victory. Lloyd George turned both Conservative backbench demands for the more vigorous prosecution of the war and frontbench apprehension at the possible upheaval of a wartime general election to his advantage, and in December 1916 he succeeded to the prime ministership at Asquith's expense.

Turner seems unsure whether to classify the Welshman's triumph as a "palace revolution" (pp. 112, 151), but he is in no doubt about the eventual consequences. Finding victory in Flanders elusive but the threat of class-based insurgency at home all too apparent, and fearful of any repetition of the Russian example, the Lloyd George coalition "began to make itself a counter-revolutionary government" (p. 195). The principal casualty of the coalition's premium on domestic security was the Edwardian party system itself, and Turner details the atmosphere of crisis in which such drastic intentions gained a measure of plausibility. In March 1918, during the great German offensive, Lloyd George's government faced the specter of defeat. This prompts Turner to suggest, in a manner reminiscent of Henry Pelling's linking of Japanese Admiral Heihachirō Togo to the expansion of the British welfare state, that General Erich Lu-



dendorff be accorded his due role in the reconstruction of British politics.

As for the implications of the coalition's counter-revolutionary impulse, in industrial relations it promoted conflict, not concord. Accordingly, the tripartite corporatist structure identified by Keith Middlemas (*Politics in Industrial Society* [1979]) figures in Turner's account as a transitory phenomenon, whose erosion in the immediate postwar years reflected the persistence of an older pattern rather than the collapse of a once durable consensus. In electoral politics it is the distorting effect of the "coupon" on the general election of 1918 and the fragmented nature of political development that emerge as the principal themes of Turner's somewhat circumspect concluding section. While he finds little evidence to suggest that the implementation of universal male suffrage in 1918 was the key to Labour's advance, he stresses the strong support given by newly enfranchised women to coalition candidates (although this was perhaps more a matter of class than gender).

Minor errors, such as the misattribution of Liverpool's Conservative Party boss Sir Archibald Salvidge (pp. 321, 330), are rare. And although reading this book straight through requires something of the stamina appropriate to its chosen subject, those who persevere will be rewarded with a fuller appreciation of the changing features of Britain's political landscape during the war. Despite the wealth of previous work on the subject, all historians of twentieth-century Britain will be in Turner's debt.

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GIULIANO FERRARI BRAVO. *Keynes: Uno studio di diplomazia economica*. (Università degli studi di Padova, Dipartimento di studi internazionali, number 4.) Padua: Cedam. 1990. Pp. 480. L. 40,000.

Giuliano Ferrari Bravo has put together in one volume a number of studies, some of which have been previously published, on John Maynard Keynes, yet unlike many books of this type there is a coherent theme and an underlying unity to the collection. Ferrari Bravo is not interested in Keynes as a theoretical economist. Instead he has examined Keynes in the context of his work within the British Treasury. The author contends that Keynes was essentially a civil servant whose views were shaped by his long-standing ties to the official governmental financial bureaucracy. Keynes served this bureaucracy as more than an outside consultant. In fact, his work as a civil servant becomes the unifying thread that brings together the various aspects of Keynes's long career. Ferrari Bravo cites with some approval Von Hayek's assessment that Keynes was never a pure theoretical economist but rather an economist whose principal aim was to influence political decisions and whose

economic theories were designed to accomplish that end (pp. 409–10).

Ferrari Bravo concentrates on three moments in the career of Keynes: his work as a treasury official during World War I and at Versailles; Keynes's role as journalist and critical outsider during the 1920s; and, finally, the loan negotiations that Keynes undertook during World War II and in the immediate postwar period. In each of these stages Keynes dealt directly and perceptively with the shifting power relationship between Great Britain and the United States. Ferrari Bravo uses Keynes's experience during World War I, when he first negotiated with the Americans, as a prelude to the far more extensive financial diplomacy in which Keynes engaged in Washington from 1941 to 1946. Especially interesting are the brief analyses of the personal relationships that Keynes had with British political leaders like Lord Asquith, David Lloyd George, and Hugh Dalton, and with the Americans Henry Morgenthau, Harry Dexter White, and Dean Acheson.

Ferrari Bravo has deliberately highlighted only one aspect of Keynes's career. There is nothing of Keynes as an economist here, and the 1930s are completely omitted from the study. The most informative chapters are those that deal with Keynes's strategies during the prolonged negotiations with the Americans for loans and credits during World War II. Throughout these negotiations, the author argues, Keynes was able to operate with a high degree of independence and initiative.

The research is extensive and current. Unfortunately, the book lacks an index and was ill-served by the copy-editing process. A number of annoying errors, such as the numerous references to the "Dowes Commission," instead of the Dawes Commission, could have been easily eliminated. At times the author seems to be caught up in minor details of the negotiating process so that one loses sight of the outcome. This is especially true of the negotiations for the major American loan in late 1945. These problems do not, however, detract from the book's merits. It offers the readers a detailed analysis of Keynes as negotiator and government adviser over a period of thirty years.

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PHILIP WILLIAMSON. *National Crisis and National Government: British Politics, the Economy, and Empire, 1926–1932*. New York: Cambridge University Press. 1992. Pp. xvii, 569. \$89.95.

In this important exploration of British politics, Philip Williamson argues that, after 1926, Britain experienced a profound national crisis that seemed to threaten its very position in the world. Putting the sterling crisis of 1931 into a broader context, he convincingly maintains that the emergency alone can-



not explain the emergence of a national government in that year. Nor does he have much patience with the view that the "high politics" of the interwar period centered on the determination to exclude from power David Lloyd George, Winston Churchill, and other former coalitionists. The national government, he contends, was formed in an atmosphere of fear of financial collapse, drastic inflation, a severe fall in living standards, and social and political disruption. It continued as a coalition because of additional fears—of militant socialism and trade union sectionalism as well as electoral defeat and the loss of political power.

By the end of 1930, many believed that the party system was incapable of dealing with the combination of financial, economic, imperial, and political problems faced by the nation. The time had come to attract various sections of opinion. The Labour government lacked resourcefulness and flexibility, not simply in its ideas but also in the way it articulated its assumptions and commitments. Despite its criticism of capitalism, its expectations relied on the successful operation and good will of private enterprise. Although it believed in the regulation of much economic activity, it was tied to the old Liberal traditions of free trade.

Ramsay MacDonald's socialism, as Williamson neatly puts it, had always been more concerned with "community" than with "class." It was about service, interdependence, integration, "rationality," and organization. It was about permitting the disadvantaged to share the benefits—and the burdens—of full membership in the national community. Like most of his Labour colleagues, he resisted the notion that the party was the instrument of the trade union movement. For all those reasons, it appeared consistent with what he had preached for many years to accept expenditure cuts in the social services within the context of "equal sacrifice." By the middle of 1931 he feared a financial collapse, which would be much worse for the working class than projected cuts in benefits and wages. And he evidently believed that by not taking forceful action, the Labour Cabinet had destroyed the party's claim to be a "party of government."

Conservative leaders, for their part, did not believe that they occupied so strong a position, electoral and otherwise, as later proved to be the case. They tried desperately to keep retrenchment from appearing to be a matter of rich against poor and thus to avoid the polarization of competing Labour (Lloyd George Liberal) versus Conservative (Simonite Liberal) alliances. Williamson is especially absorbing in his demonstration of the tentativeness and the perhaps uncharacteristic flexibility with which Neville Chamberlain approached the possibility of national government.

As for the Liberals, Lloyd George's schemes for extensive public works lacked credibility in the light of his record on unemployment and, in any case, by

the spring of 1931 his section of the party stood for retrenchment and free trade. For the party as a whole, a national government offered escape from a variety of internal tensions. The party's apparent commitment to radical economics and progressive politics revealed itself as tenuous indeed, and most Liberals had no difficulty in agreeing with their acting leaders to support "sound finance," retrenchment, and a national government. Although some had been influenced earlier by the still developing notions of John Maynard Keynes, virtually no Liberal peers, MPs, and candidates took issue on Keynesian grounds with the fiscal and economic assumptions of the proposed government.

Like other studies emerging from the so-called "Cambridge School," Williamson's work is an almost day-by-day analysis of high politics. Unlike some of the others, however, this revealing account never loses sight of the broader issues that underlay the maneuverings of the politicians who, as frequently as not, were only dimly aware of their ramifications. It is an important corrective to some of the still continuing myths about the interwar years.

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TUVIA BEN-MOSHE. *Churchill: Strategy and History*. Boulder, Colo.: Lynne Rienner or Harvester Wheatsheaf, Hempstead, U.K. 1992. Pp. vii, 397.

The ongoing fiftieth anniversary commemoration of World War II provides an appropriate occasion for rethinking our assessments of its various aspects. The principal actors have departed, the official histories have been written, the archives are (for the most part) open, and a new generation of historians can look on the war with fresh eyes. It is surprising that the revaluation of those years has not hitherto focused more intently on Winston Churchill, whose memoirs have done so much to shape popular perceptions of the war and whose legend, lovingly tended, makes his name one of the few from that era generally recognized today by the average person. Tuvia Ben-Moshe has tackled Churchill's strategic thinking in a densely written monograph of revisionist intent. He argues that, in World War I, Churchill had tried (unsuccessfully) to combine the two poles between which British strategy had traditionally oscillated, the maritime and the continental. In 1939 this was again his strategy. The collapse of France, however, and repeated demonstrations of the Wehrmacht's "fighting power" led Churchill to embrace a "peripheral-attritional" strategy. This, in turn, delayed the invasion of Western Europe so long that any chance of turning military victory into political capital—by meeting the Russians farther east—was lost. Afterward, Churchill obscured what had actually happened in his artful and immensely influential memoirs.

Despite his revisionist bent, Ben-Moshe's argu-

ments are not particularly new. Many of his criticisms of Churchill have been made before. The weakness of this type of critique is that it overrates Churchill's freedom of action and underestimates the degree to which war creates its own dynamic. Churchill was often a prisoner of circumstances—like the incoherences of coalition warfare—that were beyond his control. As Lord Kitchener remarked early in World War I, Britain had to make war as it could, not as it would have liked. Even Churchill could not change this. Ben-Moshe is on sounder ground when he suggests that Churchill's desire to delay the cross-channel attack as long as possible arose from the fact that he "had lost faith in the skill of the field high command and in the fighting spirit of 'Tommy Atkins,'" (p. 264). The evidence for this is, if scattered, cumulatively impressive. It suggests that, however important politically and psychologically Field Marshal Bernard Montgomery's carefully publicized successes were to Churchill and the British public, the prime minister did not confuse them with a renaissance of the British Army. Yet—and this is a point Ben-Moshe does not address—the British Army did get (somewhat) better as the war progressed. Had Churchill stopped noticing? The whole issue points out the need for careful analysis of the British Army during the war, rather than further sterile exchanges between Montgomery's partisans and his rather more numerous denigrators.

The raising of issues like this makes Ben-Moshe's book rewarding, albeit difficult, reading. He may fail to totally convince, but he does demonstrate the possibility of developing new perspectives from even the most thoroughly worked over material. His study also underscores the importance of Churchill's decision to get his version of the war into print quickly. We are still discussing what he wrote, which is not quite the same thing as what happened.

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BARBARA B. DIEFENDORF. *Beneath the Cross: Catholics and Huguenots in Sixteenth-Century Paris*. New York: Oxford University Press. 1991. Pp. xi, 272.

For most of the twentieth century, historians of sixteenth-century France have found ways to take religion out of the Wars of Religion, prompting many scholars from the current generation to try to put it back in. Historians like Henri Drouot offered socio-political interpretations for the seemingly religious violence that shook France between 1562 and 1595, but, as recent works like Denis Crouzet's *Les Guerriers de Dieu* (1990) demonstrate, religion did play a formidable role. Barbara B. Diefendorf's fine book, as the title reveals, also finds room for religion in understanding the violence that gripped France. It is also, however, close "to traditional history in the causative role that it attributes to nonreligious or

political factors." Indeed, she hopes that when the historiographical "dust settles," this book "will help demonstrate that there is a middle ground" (p. 6).

Nonreligious factors are important in Diefendorf's account, but one cannot read this book without being overwhelmed by the deep influence of religious passion. To appreciate the importance of religion in the Wars of Religion, Diefendorf cautions, one must avoid the logical temptation to begin the analysis with the emergence of the Holy League in the 1580s. One should direct one's attention instead to the crucial period between 1557 and 1572 when religious hatred intensified and "large-scale incidents of religious violence took place in Paris" (p. 7). Framed by the "breakdown of order" (p. 50) reflected in a religious riot on the rue Saint-Jacques at their beginning, and the "murderous paroxysm" (p. 49) that was the Saint Bartholomew's Day Massacre at their end, these fifteen years witnessed a radicalization of Protestant and Catholic opinion.

After a deft overview of Paris's economic, social, and governmental "structures," Diefendorf explores the religious world of "the Most Catholic Capital" (p. 28). Understanding the importance of religious unity for Catholic Parisians and appreciating the desperate material conditions under which many Parisians suffered, by the time we descend the slippery slope to the massacre of Calvinists in 1572, we fully comprehend how and why it could happen. Diefendorf's four-chapter narrative of the events ominously preceding and, indeed, presaging the massacre is a chilling tale about the "contagious quality of fear" (p. 92) and, under certain conditions, its shocking outcome: the sanctified murder of fellow citizens.

Midway through her book Diefendorf shifts from narrative back to analysis and examines the nature of the Parisian Protestant population. This "underground Church" (p. 107) was convinced that it lived in "the shadow of God's wings" (p. 127), and, as Diefendorf asserts based on a study of the records of Protestant notaries, secured its spiritual community by a social and economic one. This embattled minority may have been able to withstand for a time the inflammatory sermons of anti-Protestant preachers like Simon Vigor, but it ultimately had no defense against the "fury" of the Catholics who slaughtered them. How could they, when some of the city's militia joined in the massacre?

Diefendorf emphasizes religious antagonism, culminating in a massacre that was "a terrible act of faith on the part of an impassioned populace that believed itself to be executing the will of God" (p. 177). She points out, however, that the murderous religious fervor turned out to be the work of a militant minority (which makes all the more puzzling her frequent references to a general "impassioned populace" or "popular fury" [p. 68]). Many of the civil authorities were repelled by the massacre that they did not want but were powerless to prevent. Diefendorf does not conclude from this "that all magistrates became Cath-

olic moderates and eventually joined forces with the Politique party" (not all did), but she does argue that "the contrasting outlooks of the Leaguers and their Politique foes took root during the first decade of the civil wars" (pp. 174–75).

Diefendorf argues that the roots of religious conflict in those terrible years culminating in the Saint Bartholomew's Day Massacre must be examined if we are to fully understand the French Wars of Religion. A look "beneath the cross," this meticulous, magisterial, and convincing book by a leading *seizièmiste* is indispensable reading for anyone wishing to do that. It is also compelling reading for those seeking insight into "the more universal problems that occur when competing faiths stake exclusive claims to absolute truth" (p. 8).

JAMES R. FARR  
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SARA F. MATTHEWS GRIECO. *Ange ou diablesse: La représentation de la femme au XVI<sup>e</sup> siècle*. Paris: Flammarion. 1991. Pp. 495.

I have always been annoyed by reviews that start out with quibbles about a book's misleading title. Nevertheless I feel bound to say it: the reader is apt to approach this book expecting a general treatment of sixteenth-century depictions of women, especially given the broad application the word "representation" has acquired in recent years. In fact, this is a highly specialized study of sixteenth-century French prints, and the topic is further narrowed by the author's postponement of a wide range of subjects to a future companion volume: issues of marriage, sexuality, and "marginal" women appear here only peripherally. The focus, instead, is on positive and negative images of women, with emphasis on the century's highly polarized conception of female nature.

Sara F. Matthews Grieco argues persuasively for the need to devote more scholarly attention to the printed pictures of the early modern period. New technologies of printing transformed the social distribution and uses of images as well as words, spreading them to a much broader audience. The book presents an exhaustive survey, generously supplied with illustrations, of idealizations and condemnations of women in sixteenth-century prints. Topics include virgins both passive and active, images of motherhood and fertility, and such presumably female failings as pride, avarice, fraud, sloth, talkativeness, and violence. Grieco's commentary attempts to sort out class differences among the producers of images, and her interpretation draws on Robert Muchembled's conception of an elite/written/urban culture gradually suppressing the popular/oral/rural one (*Culture populaire et culture des élites dans la France moderne* [1978]), placing increased limitations on women in the process. Grieco sees this as helping to explain the hostility

and restrictiveness toward women that she finds even in positive female images.

The bulk of the book is divided into three very long chapters: one on the polarization of female images, one on virtuous women, one on female faults. Although the division of images into positive and negative is designed to underline the polarity of the society's thinking about women, it also tends to obscure some ambiguities. For example, the cover picture of Judith represents a traditional heroine, but could I be the only viewer reminded of Medusa by her serpentine coiffure, or of Amazon legends by her vestigial left breast? Although Grieco's explications are generally sound, one often wishes for a fuller analysis of individual images, with a correspondingly greater selectivity about the images and topics to be discussed. At times the book reads like a *catalogue raisonné*, with subjects introduced more to fill out a given category than to construct an argument.

The attempts at statistical analysis of the material are not very successful, in part because the evidence does not lend itself to quantification, and in part because the data are not presented well. One pair of tables (pp. 90–92) gives only percentages, with no indication of the absolute numbers involved. This makes it impossible to judge whether a difference between 2.5 percent and 5 percent is "decisive," as Grieco claims. The problem is compounded by a lack of clarity in the discussion about what the various percentages refer to. The numerical tables designed to show change over time suffer from overlapping chronological categories and insufficient numbers for the early part of the century, so they do not really support Grieco's conclusions (see especially p. 207). It would be better simply to admit that interpretation of these images is based on "reading" them, not counting them.

The iconographical treatment of early modern women is an important subject, and prints have been neglected as historical sources. This will be a useful book for specialists to consult, though few will want to read it whole, given its length and its loose thematic structure.

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CLAUDE MICHAUD. *L'église et l'argent sous l'ancien régime: Les receveurs généraux du clergé de France aux XVI<sup>e</sup>–XVII<sup>e</sup> siècles*. Paris: Fayard. 1991. Pp. 804.

"Vanities," Pierre Boël's 1663 canvas, graces the cover of Claude Michaud's new book, illustrating the fabulous riches attributed to the clergy of *ancien-régime* France. Michaud's monograph offers us the first detailed look at two fundamental questions concerning those riches: first, did the French clergy contribute substantially to the royal coffers? And second, how did that contribution evolve over time? Michaud takes an institutional and prosopographical approach

to the financial history of the French church. Examining in great detail the clergy's tenths, annuities, "free gifts," and capitations, he goes far beyond the work of such predecessors as Pierre Blet (*Le clergé et la monarchie: Étude sur les assemblées générales du clergé de France de 1615 à 1666*, 2 vols. [1959] and *Les assemblées du clergé et Louis XIV de 1670 à 1693* [1972]) or Bernard Schnapper (*Les rentes au XVI<sup>e</sup> siècle: Histoire d'un instrument de crédit* [1957]).

Michaud demonstrates that the sixteenth-century clergy provided considerable aid to the king: an annual average of 2.5 million livres in what he estimates to be a total revenue of about fourteen million livres; he settles for an estimate of 15 to 20 percent of total royal income (p. 146). A more accurate assessment of total royal revenue in the 1570s and early 1580s would be nearly double what Michaud suggests, so that the percentage of the clergy's contribution should, I believe, be estimated at about 8 to 10 percent. Michaud emphasizes three elements about this contribution: that it provided hard cash to a specie-strapped state; that the clergy's (comparatively) timely payment of annuities helped maintain stability in Paris; and that the clergy's taxes provided one of the most reliable sources of income during periods of instability. One would like to see more discussion of the first of these issues, given the importance placed on the flow of cash by other historians, such as Françoise Bayard (*Le monde des financiers au XVII<sup>e</sup> siècle* [1988]) and Daniel Dessert (*Argent, pouvoir et société au Grand Siècle* [1984]).

The clergy's percentage contribution declined sharply under Henry IV and Louis XIII; indeed, the nominal contribution only moved up again in the 1620s, with the introduction of the free gifts. The evolution of the clergy's contribution followed that of royal finances as a whole: considerable increase from the mid-1620s to the late 1630s, instability in the 1640s due to difficulties of collection, and new stability beginning around 1654 and intensifying in the late 1650s and 1660s. Although the clergy provided substantial sums of cash to Louis XIV, the percentage of its contribution remained relatively low, certainly under 4 percent. In the eighteenth century, to judge from Michaud's brief afterword, the clergy contributed progressively less and less; in the budget of 1788, its total input represented a pathetic 0.74 percent. Little wonder that the revolutionaries had such broad support to confiscate the riches of the church.

Michaud relies almost exclusively on archival materials, especially the accounts of the clergy, the notarial registers of Paris, and the registers of the Paris Hôtel de Ville. The richness of the source material paradoxically provides both the book's main strength and its primary weakness. Michaud reproduces a bewildering array of figures, citing even derisory sums in the text. The 4,000 footnotes (taking up 195 pages) offer even more figures, on transactions concerning as little as fifty or seventy-five livres.

When one considers that the seventeenth-century receivers general of the clergy had an average worth of well over one million livres, one gets a sense of the level of detail Michaud has unearthed and, all too often, reproduced.

Institutionally, Michaud finds the same patterns revealed by Bayard and Dessert: the sixteenth-century shift in financial capital from Lyons to Paris; the constant upsurge in demand for money during wartime; and the slow regularization of administration in the seventeenth century, with its culmination in the routinized structures of Louis XIV. Individually, his collective biography of the eleven receivers general, three of whom held the office for mere months, includes individuals as varied as Claude Marcel, a simple bourgeois of Paris and perhaps one of the leaders of the St. Bartholomew's Day Massacre, to Pierre-Louis Reich, *sieur* de Pennautier, deeply implicated in the Affair of the Poisons. The passages on Marcel provide the highlight of the book, because they offer a rare example of Michaud's analytical powers. His comment that "the phenomenon of clientage, especially in the town, was not the appanage of nobles" (p. 170) leads to a fascinating discussion of Marcel's clientage network among the ordinary people of his Parisian *quartier* and of the role of popular songs in preparing the way for St. Bartholomew's Day.

Alas, there are far too few such interludes in a monograph that reminds one of the worst excesses of the old *thèse d'état*. One marvels at the research and information in Michaud's book, yet comes away disappointed that so knowledgeable a historian provides so little analysis of the state-building process of the *ancien régime*.

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Georgetown University

ANNIK PARDAILHÉ-GALABRUN. *La naissance de l'intime: 3000 foyers parisiens, XVII<sup>e</sup>-XVIII<sup>e</sup> siècles*. Foreword by PIERRE CHAUNU. (Histoires; Travaux du Centre de Recherches sur la Civilisation de l'Europe Moderne.) Paris: Presses Universitaires de France. 1988. Pp. 523. 250 fr.

This remarkable work synthesizes the research of fifty-one students from Pierre Chaunu's seminar at the Sorbonne. It aims to reconstruct the sentiments, values, and aspirations of Parisians by inference from the objects in which they invested (emotionally as well as financially) and in the midst of which they passed their daily lives.

It is in the probate inventories conserved in the *Minutier central* that Annik Pardailhé-Galabrun's researchers found "the palpable testimonies of a material culture we have lost" (p. 275), and one of the work's major contributions is establishing probate inventories, alongside wills and marriage contracts, as a source in the history of cultures and mentalities.



The largest section of the book (occupying nearly half the original French version but almost entirely omitted from the recent English translation) consists of an exemplary analysis of the reliability and limitations of the document. Probate inventories are strongly biased toward the later years of the period (of 2,783 inventories, only 441 date from the seventeenth century) as well as toward older persons, the upper social strata, and the Right Bank. Most important among their limitations, they were not generated routinely on the death of a property holder but only on request for cause, so that merely 10 to 15 percent of deaths produced one. Still, these immensely detailed descriptions of objects were common enough to allow Pardailhé-Galabrun to capture one house in ten, one household in 200 formed in Paris during the seventeenth and eighteenth centuries.

Pardailhé-Galabrun locates "the fantastic revolution of the house" (p. 16) between 1725 and 1775, when the norms of habitat and living patterns underwent more change than they had in the previous two centuries or would in the next century. Her study lends precision (and some revision) to remarks of contemporary social critics who observed that household objects became more numerous, more varied, and more widely disseminated in this period; she shows the nature and rate of changes and speculates on the changes in behavior and thinking they either reflected or caused. She argues that the innovations constituted the Enlightenment in action, for many brought rationality, order, and the pursuit of happiness into daily experience. The more comfortable upright kitchen replaced the kneeling kitchen, habitations became more spacious per person, interior spaces became ordered more rationally and specialized by function. Reorganization served both sociability and privacy. Chairs much more numerous than occupants, armchairs, social games, and the generalized use of coffee all suggest a new home-based sociability. At the same time, the conversion of formerly multipurpose spaces into intimate premises suggests a new premium on privacy: *cabinets* for the satisfaction of bodily functions in privacy, *secrétaires* in which objects could be kept private, *chambres* with fewer occupants and more private beds. Handsome fabrics and color-coordinated decors suggest inhabitants' growing interest in embellishing domestic spaces.

The inventories also shed light on other issues: on shifts in social structure (material culture became similar by wealth rather than by traditional rank as the population reshuffled into classes), on the socio-professional map of Paris, on home ownership (a mere 14 percent owned their residence), on household structure (extended kin were occasionally present, but fully 26 percent of persons died in solitary households), and on family relations (both affection and tension can be found in the notaries' minutes). Pardailhé-Galabrun emphasizes, against Michel Vovelle (*Piété baroque* [1973]), the abundance

of religious objects in all social strata, but notably among nobility and elite; this, in her view, bespeaks the stability of traditional piety down to the shock of the French Revolution.

Much of the book's impact comes from the profusion of illustrative anecdotes that convey the warmth and texture of private lives. Despite the exhaustive underlying research, the presentation remains essentially impressionistic. Computerization of the data would have permitted deeper questions about the correlates and determinants of ownership, and could have moved the study from description to explanation. Did material culture change because of new mentalities, or simply because greater wealth and technological progress made age-old desires attainable? Statistical analysis would have complemented the work's anecdotal richness.

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ROBERT S. WEDDLE. *The French Thorn: Rival Explorers in the Spanish Sea, 1682–1762*. College Station: Texas A&M University Press. 1991. Pp. xi, 435. \$49.50.

This book is the second volume in Robert S. Weddle's intended trilogy describing three centuries of discovery and exploration around the entire Gulf of Mexico. As such, it serves as sequel to his *Spanish Sea: The Gulf of Mexico in North American Discovery, 1500–1685* (1985); the new book also stands on its own, however, as an exhaustive narration of French activity on the northern Gulf Coast and the consequent imperial tensions with Spain to 1762. The exploits of René Robert Cavelier, Sieur de La Salle, and his countrymen aroused concern among Spanish officials from Mexico City to Madrid, forcing them to focus their attention less on the Mississippi River, gateway to the continent, and more on eastern Texas, the territory most subject to French incursions. This attractively produced and well-organized monograph is based on extensive research in Spanish, French, and North American materials. Moreover, it includes some twenty French and Spanish maps from the seventeenth and eighteenth centuries, which offer fascinating insights, carefully developed in the last two chapters, into the degree of cartographic confusion that prevailed among virtually all European explorers.

Indeed, according to Weddle, the early French and Spanish adventurers were hardly heroic, larger-than-life, or even competent. The opening chapters give passing credit to La Salle for skills such as his mastery of eight to ten Indian languages, but repeatedly fault his style of leadership along with his men's capacity for discipline. A witness to La Salle's last years recorded, "This is a man who has lost his mind," but Weddle characterizes the explorer as a manic depressive who would have benefited from modern-day drugs like lithium carbonate (pp. 26–27).

In 1682, La Salle had reached the Gulf of Mexico



from Canada via the Mississippi River. But when he tried to find the mouth of the same waterway by approaching through the Gulf on a subsequent expedition from Saint-Domingue, the Frenchman and his followers ended up lost ashore in eastern Texas. Instead of rediscovering the Mississippi, La Salle made it inland to the Rio Grande. Then he was murdered by an unhappy subordinate while the French colony left behind on the coast fell victim to hardship, disease, and Indian treachery. Yet Spanish policy makers, indecisive and self-defeating in their Old World deliberations, never did appreciate the magnitude of French failures. Instead, a legacy of suspicion and fear prevailed for another eighty years, as Spain had to spend ill-spared resources to maintain a defensive posture that did not permit adequate exploitation of the Mississippi River.

The middle chapters further chronicle Spain's weakness and bad luck as it sought to protect precious Mexican silver mines from imagined threats from interlopers in Texas. Thanks to perpetual manpower shortages, the influence of shipping interests vested in Veracruz, and the blind fear that uncontrolled trade would invite foreigners and enable the evasion of Crown revenues, Spanish officials did not open new ports on the northern Gulf Coast. Instead, they relied on the French to serve as a buffer between them and the increasingly active English. After 1762, however, when Spain lost Florida and acquired Louisiana, the opportunity once again existed for the Gulf to become a "Spanish Sea." The definitive account of this final stage of comparative colonial rivalries awaits completion of Weddle's next volume.

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ELISABETH ROUDINESCO. *Théroigne de Méricourt: A Melancholic Woman during the French Revolution*. Translated by MARTIN THOM. New York: Verso. 1991. Pp. x, 294. \$34.95.

Whether described as courtesan, feminist, Amazon, or madwoman, the image of Théroigne de Méricourt is very much a part of the revolutionary myth. Elisabeth Roudinesco's biography is a welcome addition to contemporary scholarship on women in the French Revolution. Through a psychoanalytic approach, she offers a challenging interpretation of Théroigne's involvement in revolutionary politics and her ensuing madness, while at the same time exploring issues of revolutionary feminism and the birth of the modern asylum.

Roudinesco frames the discussion of her heroine's life as a "dialectic of confinement and liberty, of reason and madness, and of exile and return" (p. 150). The daughter of wealthy peasants from Marcourt in the Ardennes, Anne-Joseph Terwagne had a difficult youth. Roudinesco enumerates the "humili-

ations" she experienced: an initial seduction and abandonment, the birth and death of a baby, a failed career as a singer, and a bout with venereal disease. As a demimondaine, Terwagne suffered, her biographer claims, from the common feminine malady of ennui or melancholy.

The revolution represented liberation and an idealized love for the twenty-seven-year-old woman who became known as Théroigne de Méricourt. Donning masculine attire to attend sessions of the National Assembly, Théroigne fraternized with men such as the mathematician Romme and served as archivist for the short-lived *Société des amis de la loi*. Ill-founded rumors about her involvement in the October march to Versailles, however, drove the "belle Liégeoise" into exile. In Liège, Théroigne's association with French patriots led to her confinement (literal, this time) in an Austrian prison, charged with being a spy for the French government. Roudinesco skillfully unravels both the narrative and significance of this episode, arguing that her prison writings reveal how the revolution provided Théroigne with a sense of place and family.

Her liberation and return to Paris in January 1792 enabled Théroigne to plunge into Girondin revolutionary politics, calling for war, tribunals of women to reconcile citizens, and the creation of legions of Amazons. But as the Jacobins gained ascendancy, Théroigne's brand of "warrior feminism" was replaced by that of the sans-culottes women, with whom she had no affinity. Following a whipping by these same women, Théroigne began to lose her grip on reality.

Théroigne was committed to an asylum in 1795, where she stayed until her death in 1817. Roudinesco's finest pages address how the specialist on mental health, Étienne Esquirol, presented Théroigne in order to show that the revolution was madness and thus propelled her into incurable insanity. The biographer disagrees. Using Sigmund Freud's insights, she argues that Théroigne's evolution from melancholy to psychosis occurred "through the loss of the ideal object, the Revolution, with which she had identified" (p. 229).

Although Roudinesco strives to contextualize Théroigne's life, her efforts are not always successful. Her analysis of women during the revolution ignores recent Anglo-Saxon scholarship and offers a misleading chronology of revolutionary feminisms. Many historians, for instance, would disagree with her argument that the Society of Revolutionary Republican Women represented a less militant form of feminism due to its respect for separate spheres. Roudinesco is at her best when she sticks close to her subject, offering the reader lengthy quotations from which the myth of Théroigne has been constructed. The last chapter addresses how historians, mental-health specialists, and even playwrights have depicted Théroigne.

Given Roudinesco's interest in images, it is surpris-

ing that she largely neglects the rich iconographic record recently uncovered by Claude Langlois (*La caricature contre-révolutionnaire* [1988]). More problematic, Roudinesco's use of footnotes is sloppy and the sources for her supposedly more accurate reading of Théroigne's personality are left unclear. Despite these flaws, the translation of Roudinesco's book offers the nonspecialist a fascinating glimpse into the life of an extraordinary woman and the ways that men have represented this life.

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CLAY RAMSAY. *The Ideology of the Great Fear: The Soissonnais in 1789*. (Studies in Historical and Political Science, number 2.) Baltimore: Johns Hopkins University Press. 1991. Pp. xxxi, 311. \$45.00.

About 7 o'clock on Monday morning, July 27, 1789, a messenger hurried into Clermont to find the commanding officer of the territorial police and tell him that in the village of Sacy-le-Grand 200 persons were cutting down the ripening grain. In quick succession other messengers arrived. They had heard that the crops in Estrées-Saint-Denis and elsewhere had been ravaged. The police commander led a mounted detachment out to investigate and found that the reports were false. But the rumor spread far and wide. Villages near Clermont were aroused; hundreds of men armed with pitchforks, axes, and other implements came into Clermont to help defend the town. On the same morning, thirty kilometers away, in northern Valois, the village officials of Saint-Martin-de-Béthisy sent out a warning that bands of brigands were spreading to cut down the crop on the adjacent plain. This rumor followed a path different from that which reached Clermont but it, too, passed through many localities. These were the beginnings of the Great Fear in this region some sixty kilometers north and northeast of Paris.

On the same day, paying no attention to the rumors and panic going on all around them, inhabitants of five villages north of Noyon broke down the doors of the château of Frétoy, drank the wine, and carried off the grain they found there.

In 1932, in *La grande peur*, Georges Lefebvre described and interpreted these panics all over the country. He distinguished sharply between a panic and an uprising or an attack on a château: where there had been an uprising, as a rule, there was no panic. He connected the panics to economic and psychological conditions (notably the universal and chronic fear of brigands) in the countryside, and related them to fears of an "aristocratic plot" engendered by the revolution.

Clay Ramsay's book is only the third on the topic in two generations. It is especially welcome for its thoughtful discussion and for its many stimulating observations. On some points Ramsay disagrees with

Lefebvre, but mainly he strives for a deeper understanding. He treats the panics in Clermontois and Valois and the attack on the château of Frétoy as the starting point for a study of the social relations, beliefs, and attitudes ("ideology" in Antonio Gramsci's sense of the term) that made the Great Fear possible and shaped its results.

The panics are a puzzle because so many people believed the rumor and acted on it. Authorities tried to supervise the mobilization of people, and in some places they sent out warnings that spread the rumor. The Great Fear was urban as well as rural, and was transmitted deliberately as well as through excited talk and hasty action.

Above all, the Great Fear was about the food supply. Ramsay devotes solid chapters to the system of cultivation of grain, the markets, and noncommercial distribution to beggars. It becomes clear how protection of the unharvested crop could mobilize people to cooperate across class lines and across the rural/urban divide.

The threat was supposed to come from "brigands." In an effort to pin down the meaning of the term, Ramsay collected ninety-nine examples of its use in documents in 1789. It was usually not a euphemism for "lower class" or "poor," but instead referred confusedly to outsiders: those outside the locality, outside the grain-cultivating economy, or outside the law-abiding community.

The Great Fear threatened public order and led to the formation of many new local militia units. Ramsay analyzes at length what this meant. He steers clear of any sharp dichotomy between state and civil society, and thus avoids making revolutionary politics seem static or incomprehensible. He explains the relations between three principal actors: the rural communities, the towns with their law courts, and the agents of central government. He examines the role of the new militia in those relations. Finally, drawing on a discourse by Robespierre from 1790, he outlines the possible consequences of arming a population. In the end, his study illustrates how the political configuration of the *ancien régime* was transformed into a dynamic and much more problematic situation with potentialities for revolutionary change and sharp social conflicts. There is much to learn and ponder in this excellent book.

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CARLA HESSE. *Publishing and Cultural Politics in Revolutionary Paris, 1789–1810*. (Studies on the History of Society and Culture, number 12.) Berkeley and Los Angeles: University of California Press. 1991. Pp. xvi, 298. \$29.95.

Book historians have stayed clear of the French Revolution, putatively for lack of documentation. The disappearance of the *ancien régime's* Administration of the Book Trade meant a hiatus in censorship records, book historians' life line. Only exceptionally do we find the void filled locally, as by Frédéric Barbier for Strasbourg (*Trois cent ans de la librairie et d'imprimerie: Berger-Levrault, 1676-1830* [1979]). Carla Hesse's research goes far beyond what one might expect. Beginning with the unraveling of the complicated censorship apparatus between 1788 and 1790 and the termination of the Paris Book Guild, admittance to which was limited to thirty-six printers on the eve of the revolution, she takes us into the deregulated world of publishing in the 1790s.

Competition was cutthroat; gone was the *privilege* that had protected the publishing profession before 1789. Related to the exponential multiplication of publishers and printers was the proliferation of ephemera, particularly newspapers and pamphlets. These drove out books like *assignats* drove out *livres*. Pirate editions of French classics, moreover, made it impossible for a first publisher to survive without effective copyright laws. Bad publishers drove out good ones.

Authors faced considerable difficulties getting protection for their literary rights. Diderot had sought a place for literary property by which the author would have been entitled to full enjoyment of copyright. Condorcet later wanted the author's creation to go on the public domain as soon as possible, since he felt ideas did not belong to anybody. The landmark legislation of July 19, 1793, gave authors proprietary rights over their work during their lifetimes and to their heirs for five years after their deaths. Effective protection was still not forthcoming. Bernardin de Saint-Pierre was among those who witnessed pirate editions of his works.

Book publishing as a whole slid from a peak of 1,700 volumes in 1788 to 300 in 1796. Bankruptcies rose. For the whole revolutionary-imperial period, there were 337 printers and 887 publishers, clearly not all in existence simultaneously. Hesse has used the voluntary *dépot légal* at the Bibliothèque Nationale after 1793, made obligatory by 1810. The latter marked a new regulation and recentralization made effective after 1800. The combination of economic anarchy and intellectual mediocrity forced the whole publishing world to rethink their revolutionary experience.

Similar reexamination was going on in theatrical circles after the Le Chapelier Law of 1791 deregulated the theater world, a comparison Hesse fails to make. The substance of both drama and books drifted toward melodrama and the *roman noir*. This romantic fiction appalled the deregulators who believed, like Thomas Jefferson, that truth would rise to the surface in the free marketplace of ideas. Like the theater industry, publishers had come to realize that regulation was desirable, as no respectable publisher

or printer seemed to be able to prosper in a deregulated market. Limitations of the numbers of producers, and strengthening of authors' rights and the restoration of censorship, first by Napoleon's new ministry of police under Fouché and then by the establishment of the Direction of the Book Trade in 1810, seemed to be what was required.

Hesse seems not to recognize the latter as simply a restoration of an *ancien-régime* institution. The state's effectiveness was far greater. While neither privilege, strictly speaking, nor corporation was restored, censorship prior to publication (distribution) was reinstated. The result was a drastic reduction of ephemera and a successful strengthening of the book trade, particularly for classics. The reading public was thus depoliticized until the Restoration. Hesse notes that it was not the author as cultural hero of 1793 who emerged from the revolution, but the author seen as a link in the familial and national patrimony, protected at first by an extension of the copyright in the family to twenty years after his or her death. This did not eliminate clandestine publishing, however, for Hesse ends her book with a telling description of a Grub Street publisher, Jean Gabriel Dentu, who successfully evaded a police inspector.

While Hesse sees the salutary effects of censorship in rescuing the beleaguered publishing industry, she offers no general conclusion about censorship and liberty of the press. Like political freedom, it seemed not to have worked out in the short run.

This is an excellent book because of its enormous archival finds, because of the discipline, lucidity, and thoroughness of the exposition, and because of its factual fidelity. It is, in my opinion, one of the best contributions of the present generation of American scholars to French Revolutionary history.

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GAIL BOSSENGA. *The Politics of Privilege: Old Regime and Revolution in Lille*. New York: Cambridge University Press. 1991. Pp. xi, 263. \$44.50.

Over the past twenty years a wave of historical revisionism has effectively dismantled the traditional interpretation of the French Revolution as the victory of a rising bourgeoisie over the combined forces of a despotic monarchy and a reactionary aristocracy. The revisionists have portrayed the revolution as an essentially political event rather than a social revolution, almost the accidental result of a fiscal crisis bungled by an incompetent monarch. Gail Bossenga challenges the revisionist interpretation by arguing that the revolution was the logical, if not inevitable, result of fiscal contradictions within the *ancien-régime* corporate structure, and that it can also be characterized, at least in the case of Lille, as a bourgeois revolution in political if not in economic terms.

The book is arranged as a series of case studies,

focusing on the relationship of a variety of corporate institutions—the *Bureau des Finances*, the province of Walloon Flanders itself, the oligarchic town council of Lille, and several Lille guilds—to the monarchy and to social and political trends in the eighteenth century. Bossenga chose Lille as the urban focus for her study in order to examine the effects of state centralization and capitalism on corporate structures, Lille being the center of a *pays d'état* and a prosperous commercial and textile town. At the heart of her story, for there is a richly detailed story here, is the ongoing and heightening tension between the always growing fiscal needs of the French monarchy and the political and economic self-interest of the various corporate groups under study.

The author reveals a number of interesting paradoxes. For example, the *Bureaux des Finances* were provincial courts that had become essentially superfluous by the eighteenth century, but they were still useful to the crown as venal offices that generated substantial revenue. But as the monarchy, or its ministers, tried to wring increased revenue from these offices, and then under Lamoignon sought to suppress them entirely, the predominantly noble judges in at least some of the courts, including Lille, tried to form a national coalition of resistance. In doing so, Bossenga argues, “the idea of egalitarian citizenship was emerging within their corps” (p. 63). Thus, in this instance a challenge to corporate privilege came from within a corporate structure and not from those excluded from it. Similarly, the most serious challenge to the privileged status and power of the municipal *baillis* of Lille came first from other nobles, and not from commoners.

In examining the guilds of Lille, Bossenga observes that merchants and manufacturers actually defended their corporate privileges in 1789, rather than attacking them as obstructions to capitalist accumulation. Although in some trades, as in woolen production, merchants viewed the guilds as a restraint, in thread manufacturing the guild structure allowed for considerable growth over the eighteenth century and protected Lille merchants from rural competition. Bossenga concludes that “the same merchants who opposed corporate restrictions on manufacturing were quite willing to support the privileges of corporate guilds that helped them to protect their markets” (p. 136). Corporate privilege, in other words, was alternately defended and attacked by the same people, depending on personal self-interest and the specific situation. It was perfectly possible for members of the Lille elite to be politically liberal and economically conservative in their attitudes about privilege at the end of the *ancien régime*.

This is an impressive, well-researched, and well-argued study that will force us, or allow us, to reevaluate our understanding of the collapse of corporate privilege during the revolution. At times, perhaps, the author takes her argument a step too far. For example, in concluding her discussion of the

*Bureaux des Finances*, she argues that while “the idea may never have been fully articulated, the way was being paved toward a notion of popular sovereignty” (p. 69). This may be intellectually logical, but it is hard to believe that any of the judges of the *Bureaux* would have been comfortable with the popular sovereignty of 1793. And, in concluding that one of the central issues of the revolution was how to make the “exercise of political power accessible, principled, and equitable” (p. 201), Bossenga perhaps makes her Lille elites sound too high-minded and idealistic. But certainly she is right in emphasizing the central paradox of a monarchy that was dependent for its revenues on the very corporate bodies that blocked the way to rational economic, political, and fiscal reform in the name of their corporate privileges.

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ANNIE MOULIN. *Peasantry and Society in France since 1789*. Translated by M. C. CLEARY and M. F. CLEARY. New York: Cambridge University Press. 1991. Pp. xxiii, 247. Cloth \$49.95, paper \$14.95.

Anyone capable of reducing the very complicated history of the French peasantry since the eighteenth century to fewer than 200 pages of lucid narrative and analysis deserves our applause. Georges Duby and Armand Wallon's *Histoire de la France rurale* (vols. 3 and 4 [1976]) covers the same period and the same range of peasant problems, but takes over 1,200 pages to do so. What Annie Moulin has done is to summarize this monumental work concisely but without condescension, while incorporating relevant studies that have appeared since its publication.

Moulin's book is an up-to-date synthesis, not the place to look for new departures. In her overall interpretation, the author follows the new conventional wisdom that, prior to the genuine agricultural revolution of 1955–70, slow evolution, but not stagnation, marked French agricultural history. Throughout her book, Moulin is absolutely fair-minded in pointing out problems of historical interpretation and areas of uncertainty. Only rarely does her even-handedness become interpretive abdication, as when, for instance, she presents two incompatible interpretations of the Dorgères peasant protest movement of the 1930s essentially without comment (p. 149). I would also argue that her broad periodization of the French peasantry based largely on political regimes is excessively conventional: as far as the peasants were concerned, for example, there was very little commonality between the revolutionary roller coaster and the stable agricultural prosperity that followed it under the empire. Yet they are subsumed under a single chapter entitled “From the Ancien Régime to the Restoration: 1789–1815.”

If its dust jacket may be believed, this book is intended for English-speaking students. Such stu-



dents have indeed been accommodated by an eleven-page outline chronology and a very sketchy guide to further reading in English. Each of the five substantial chapters opens with a summary paragraph intended to orient the reader to what may become a welter of illustrative detail. The organization is straightforward, with chronological chapters divided into clearly labeled topical subsections. No student could complain that she or he "does not know where it's at," although they should complain about unusually inconvenient footnoting.

Yet American undergraduates would have to overcome a high irritation quotient. Moulin's book is sprinkled with case histories located in unfamiliar provinces and *départements*, as well as in many of the 400-odd *pays* recognized in France. None of these are otherwise identified, nor is there any map to which the perplexed may turn. I also wonder how many undergraduates would catch on that, among French academics, "Malthusian practices" (p. 61) is a euphemism for artificial birth control that Malthus neither practiced nor advocated, particularly when on the following page "Malthusian philosophy" refers to the English demographer's authentic concern with population outstripping resources. There are other stumbling blocks. What are students to make, for example, of these successive statements: "The average size of farms differed little from the average size of property blocks. Non-farming landowners would rarely let their land in a single block to farmers" (p. 12). My conclusion is that, in the real world, this book will be most useful to French history teachers wishing to broaden their insights (and lectures), to scholars in non-French peasant studies painlessly seeking a comparative perspective, and to history graduate students.

The translation by M. C. Cleary and M. F. Cleary is workmanlike, but to my taste hews too slavishly to the cadences and syntax of French academic prose. There are occasional infelicities like "animal sellers" instead of "cattle dealers" for what I assume was *marchands de bestiaux* (p. 17), occasional conflicts between British and American usage (for both French and British, but not Americans, *tarifs* or "tariffs" refer to railway rates; see p. 55) and occasional bloopers (as on p. 35: the French *déception* means disappointment and never deception).

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JEAN IMBERT *et al.* *La protection sociale sous la Révolution française.* (Comité d'histoire de la sécurité sociale.) Paris: Association pour l'Étude de l'Histoire de la Sécurité Sociale. 1990. Pp. 567.

This history of social welfare assistance examines institutions that provided aid to the destitute and infirm. It describes these institutions and their admin-

istration and analyzes the changes they underwent. It also discusses the changing ideological justifications for granting assistance. It brings together the work of several historians. Jean Imbert contributes two chapters, one on the *ancien régime* and another on the Directory; Louis Trenard provides a chapter on the Constituent and Legislative Assemblies; and Jean-Paul Bertaud offers a chapter on the Convention. Two short sections, the first by Jean Tulard devoted to the Napoleonic period and the second by Guy Thuillier on the nineteenth century, are appended to the chapter on the Directory. In addition, local studies of only a few pages by regional specialists on the Rhône, the Rouen region, the Bas-Rhin, the Nièvre, the Puy-de-Dôme, Toulouse, and the Nord appear at the end of the chapters on the Convention and the Directory. The sources include the records of individual institutions, local case studies, debates and committee reports from national assemblies, ministerial circulars, and the works of contemporary theorists and commentators on the problem of poverty.

Imbert shows that under the *ancien régime* efforts to meet the wide variety of needs of the destitute and infirm were manifested in a great diversity of institutions. Most assistance came from the municipalities and the crown working with religious orders and the church. Private charity was also funneled through the church. In general, each institution had its own board consisting of both lay and clerical members.

According to Trenard, under the Constituent and the Legislative assemblies the administration of social welfare assistance was secularized and nationalized. Along with the church's property the state confiscated the property of charitable institutions and created departmental commissions to integrate and administer assistance financed by allocations from Paris. Whereas formerly assistance had been justified as an opportunity for Christians to demonstrate love and purity in heart, irrespective of the sinfulness of the needy, now assistance took on a secular and materialist quality. A sense of humanity demanded that as long as some had more than enough to sustain themselves, they had a responsibility to share it with those who were unable to provide their own basic needs. The poor came to be seen as deserving assistance only if they had first done everything in their power to meet their own needs.

Bertaud argues that the Convention further centralized the administration of assistance. Committees in each commune would inscribe the names of the needy in a grand *Livre de la bienfaisance nationale* (Book of National Welfare) and the minister of the interior would make allocations to be administered by the same communal committee on the basis of these inscriptions. In addition, the Convention permitted poor hospitals and workhouses to keep any property that had not yet been sold to support their activities. Assistance also came to be justified as a right of all citizens. The Convention expanded the category of those who had a right to assistance to include those



who had made sacrifices for the nation or their dependent survivors. The Convention also extended the responsibility of the state to provide social and economic security to its citizens.

Under the Directory, Imbert demonstrates that the trend toward secularization and centralization was somewhat reversed. The right of the needy to assistance was no longer recognized by the constitution, nor was the duty of the state to assist the poor. Responsibility for assistance was given to boards for each canton appointed by the prefect with the approval of the minister of the interior. Religious orders were officially reintegrated into the operation of many social welfare institutions. Financing no longer came from Paris, but was made the responsibility of the new local boards. It came from property preserved since the *ancien régime* and the revival of *ancien régime* levies, a tax on concerts and plays and the *octroi*.

Tulard argues that continuity characterizes the history of social assistance between the Directory and the Napoleonic period. Thuillier presents the principles and institutions of social welfare in the nineteenth century as a real departure from those of the revolution, until 1880. For Thuillier the break between the revolution and the nineteenth century came in 1795 rather than in 1799.

Although this book provides a comprehensive account of efforts to help the needy, its authors do not share a common theoretical perspective. The unifying theme is simply that, despite the good intentions of administrators and legislators, assistance was always insufficient to meet the needs of the destitute and infirm.

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DAN WARSHAW. *Paul Leroy-Beaulieu and Established Liberalism in France*. DeKalb: Northern Illinois University Press. 1991. Pp. xvi, 251. \$30.00.

The resurgence of liberal sentiment in France in the 1980s and the accompanying decline of Marxist influence has revived interest in the leading French economist of the later nineteenth century, Paul Leroy-Beaulieu. His massive economic works had earlier slipped into oblivion, and he was remembered only as the author of an influential work on imperialism. Leroy-Beaulieu was unquestionably the most brilliant and influential member of the dominant group of orthodox French liberal economists of roughly the years 1860 to 1914. He was not an original thinker and made no significant contribution to economic thought. But he was an accomplished synthesizer and an incredibly prolific writer. At the top of his profession he became the ardent defender and principal spokesman of capitalism and the *grande bourgeoisie* then coming under increasing political and intellectual attack.

Dan Warsaw's biography does justice to this rich and important subject and represents an important contribution to the study of French liberalism. This is an intellectual biography that deals essentially with the development of Leroy-Beaulieu's thought skillfully integrated into the changing climates of opinion. Warsaw has apparently not seen Leroy-Beaulieu's private papers, and regrettably the man remains distant and hazy. At the outset Warsaw admits his partiality for the liberal reformers Leroy-Beaulieu so strenuously opposed, but maintains throughout the objective fairness of the conscientious biographer.

Much of the book is a detailed and lucid analysis of Leroy-Beaulieu's *oeuvre*. But in the process Warsaw probes into the core of the then liberal ideology and its increasingly bitter inner conflicts. Students of liberalism will prize his brilliant last chapter, "Leroy-Beaulieu and Established Liberalism in the History of Liberalism."

In the more than fifty years of active scholarship in a Europe of accelerated change, during which he studied closely the events and issues of the times, took clear-cut positions, engaged in an infinity of polemics, and absorbed the currents of thought of the age, Leroy-Beaulieu stood still intellectually. He remained stubbornly faithful to the thought of his youth, that of early nineteenth-century liberalism. His unshakable certainty did not secure him from the fears in his mature years of a liberal society, of civilization threatened by the revival of socialism. An even greater threat, he imagined, emerged from the dangerous political ambitions of the middle and lower strata of the bourgeoisie spearheaded by the radical republicans who had displaced the elite *grande bourgeoisie* from political office. Radicals now stood behind a state eagerly expanding its powers of interference and control of society.

A fascinating feature of this biography is Warsaw's adroit elaboration of a philosophy of history from scattered elements of Leroy-Beaulieu's works. Especially illuminating is the comparison of the key elements of his economic views to those of the opposing liberal reformers. The immutable universal laws of nature, an unchanging human nature, and an unvarying economic human are some of the elements of that philosophy of history. Leroy-Beaulieu went even further than Marx, insisting that "material self-interest and rational economic calculation" lay at the center of human action throughout history.

He had a low opinion of human nature. Humans, in general, were born lazy and were little inclined to effort. Here the discipline, drive, and ambition of the bourgeoisie in the long run made civilization possible and justified that group's predominance.

The tragic dimension of Leroy-Beaulieu's long career lay in his inflexible fidelity to his economic faith. Repeatedly, as Warsaw shows, he recognized the validity of some insight or argument or ideological opponents. But he never followed through, always falling back to bedrock certainties that rendered

impossible his ambition of becoming a creative and original economic thinker. It equally impaired his ability to propose realistic solutions or approaches to fundamental social and economic problems.

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MARJORIE MILBANK FARRAR. *Principled Pragmatist: The Political Career of Alexandre Millerand*. New York: Berg; distributed by St. Martin's. 1991. Pp. xi, 432. \$80.00.

Alexandre Millerand (1859–1943) pursued an active political life in the Third Republic for fifty-five years, a term spanning most of the republic's existence. Beginning in 1885, he was elected as a deputy from Paris consecutively for thirty-five years. President of the republic from 1920 to 1924, he concluded his career as senator with the fall of France in 1940. This work by Marjorie Milbank Farrar is the first scholarly study of a life well worth examining. It is a narrowly political biography, based on Farrar's statement that even his personal papers reveal little of the private man. Nonetheless, the narrative exposes his personality: aloof, not given to corridor politicking, a skilled orator, legalistic in his approach to issues, a master of organizational detail and administrative reform.

Within these limits, the author confronts an extraordinary passage. Beginning with an attachment to radical politics, he converted to socialism and, in his Saint-Mandé speech (1896), outlined a broad socialist program. He became celebrated as the first socialist to hold cabinet office, in the Waldeck-Rousseau ministry of 1899. The *cas Millerand*, as it became known, led to the party rule of abstention from governmental participation, and Millerand's departure from the Socialist Party. Since this phase of his career has already been studied by Leslie Derfler, Farrar concentrates, in extensive detail, on subsequent events. She argues that Millerand's path from socialism to leadership of the nationalist Bloc National in 1919–20 and the presidency is marked by consistency, the "principled" of the title. The politician who as minister of public works stressed social reform, reduced the legal working day, and urged arbitration to avoid strikes (both 1899–1902 and 1909–10), became, as premier in 1919–20, a breaker of strikes in the national interest. It is clear even before the war that nationalism is the one consistent thread in his conduct. Revealingly, as minister of war (1912–13), and particularly during the first year of the Great War, Millerand deferred to the military command (Gen. Joseph Joffre) on questions of strategy, contenting himself with lesser issues. Parliament's insistence on civilian supremacy forced his resignation and eventually brought Joffre's downfall as well.

But the climate of 1919 left Millerand poised to lead the Bloc National. Bedeviled by German resistance and British doubts, Millerand pressed forward

with insistence on German compliance with terms of the Treaty of Versailles. The hard line brought his election as president in 1920. While determined to assert a strong executive role, something that he had long preached as necessary for France, the same problems pursued him. Ultimately, his interference in foreign policy issues, whether regarding Germany or Syria, brought his resignation in 1924, in the aftermath of the electoral victory of the *Cartel des gauches*. He, rather than the "hesitant" Raymond Poincaré, is given responsibility for the Ruhr invasion in 1923 (pp. 336–38). Succeeded by "incompetent leaders" (p. 402), as senator he played the role of a Cassandra, a French "Churchill" (p. 377), which is an overstatement.

Beyond stressing the importance of biography, Farrar cites approvingly the notion that the historian should not judge (p. 2). Indeed, "perhaps" and "maybe" are frequently used as qualifiers to explain his motivation. Yet the tone of the work constitutes a defense within the restricted focus. Millerand's political career was supported by an active and successful legal practice. As minister of war he turned over to the *Comité des Forges* a virtual monopoly on metallurgical contracts (p. 164). Since 1907 Millerand had been chief legal counsel for M. de Wendel, president of the *Comité* (p. 175). Moreover, the *Comité* would become an important player in postwar Rhineland policy. No relationship is established between Millerand the politician and Millerand the lawyer.

Specialists will turn to this work for a thorough examination of Millerand's tenure, based on careful archival research. Why he acted as he did, principled or pragmatic, requires closer analysis.

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GWENDOLYN WRIGHT. *The Politics of Design in French Colonial Urbanism*. Chicago: University of Chicago Press. 1991. Pp. viii, 389. Cloth \$65.00, paper \$24.95.

The urban planners and architects who busily occupy the pages of Gwendolyn Wright's extensive study were of two worlds and also of two minds. While often sensitive to the cultural environment in which they found themselves, they nonetheless planned and imposed their designs as French imperialists, unhesitatingly convinced that they possessed the knowledge of what was fit for the urban environment of the regions submitted to French rule.

In examining the French urban effort in Morocco, Indochina, and Madagascar, the places that Wright argues offered imperial planners the best opportunities, attracted the most attention, and witnessed the most significant results, she provides a highly commendable history of the fixed-in-stone realization of French architectural design that made French colonial theory visible. Like few other works of this

compelling genre of cultural urban history, her book is a carefully considered assessment of formative intellectual currents as well as of the specific training and structural realizations of those talented architects who sought to create in the colonies something of what they imagined the well-formed city of the future should be. If not so stylistically compelling as David J. Olsen's *The City as a Work of Art* (1968) or so richly layered as Carl E. Schorske's *Fin-de-siècle Vienna* (1961), this book is an excellent synthesis that should occupy a prominent position in the literature of modern European imperialism.

Finely textured and well balanced in resources used and judgments made, Wright's book presents its thesis carefully and convincingly. The urban order, like the colonial one for which it was a template, consisted in large measure of schemes and values considered in or for the *métropole*. These could be transposed to the colonies because they provided an environment more amenable to alteration, dominated and directed as they were by the foreign reformers of the French colonial administrations.

Long interested in the subject matter myself, I now rejoice that a person so qualified, so skillful, and so appreciative of its dimensions has taken up this subject. Every attentive student of modern European imperialism will see in Wright's study a remarkably well-designed triptych of historical landscapes.

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EDWARD BERENSON. *The Trial of Madame Caillaux*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xii, 296. \$25.00.

On March 16, 1914, Henriette Caillaux shot and killed Gaston Calmette. He was the editor of the influential Paris daily *Le Figaro*, she the wife of Joseph Caillaux, formerly premier, then minister of finance and leader of the left-of-center Radical Party, against whom Calmette was waging a journalistic campaign of unmatched vilification. Charged officially with premeditated murder, and unofficially with censorship by the bullet, Mme. Caillaux was tried during the last two weeks of July 1914, the jury delivering its verdict of innocent, for a deed she never denied committing, as France faced mobilization for World War I. The Caillaux affair was spectacular drama and still makes fascinating reading—and researching—today, almost eighty years later: Edward Berenson's account is the fourth in fifteen years.

Despite the title he chose, Berenson is interested in far more than the trial. He treats the Caillaux affair as a framework on which to hang a treatment of gender-charged issues that he asserts lay behind the case, the verdict, and the mentality of the *Belle Époque*. Thus, there is an extended discussion, based almost entirely on secondary works, about the changing definition of women, the evolving morality of marriage and di-

vorce, the persistence of dueling and antique masculine ethics, the growth of psychological theories to explain crime, depopulation, alcoholism, and other social pathologies, and the role of newspapers in fomenting controversy over these matters. As a portrait of the *Belle Époque* and a summation of recent work by social and cultural historians investigating the Third Republic, this synthesis has value, but the relationship to the trial itself is tenuous.

No sensational criminal case, especially one in which national figures are involved, takes place in an ideological vacuum, but whatever the mentality of public opinion at large, Henriette Caillaux's acquittal came through the decision of twelve particular Frenchmen acting as her jury. Any interpretation of their verdict, which was rendered without commentary, is conjecture. Berenson is surely on shaky ground for a historian when he writes, "It seems likely that jurors were affected" (p. 33) and "perhaps some [jurors] harbored secret hopes" (p. 36). His pronouncement that Joseph Caillaux suffered from bipolar disorder is psychoanalysis from beyond the grave of a man who left few private papers and whose memoirs are generally agreed to be bravado. The standard of proof is no higher—a tendentious reading of the transcript—for Berenson's principal analysis of the trial, that the defense portrayed Henriette Caillaux as a woman helplessly in the grip of emotion when she shot Calmette, and therefore guilty merely of an excusable "crime of passion," because she feared that he would publish intimate letters proving her an adulteress; for the defense had to conceal that she was, in fact, a supreme example of the so-called "new woman," able to fend for herself and, as she raised her pistol, determined to punish Calmette for the damage he had already done, as well as that he planned to do, to herself and to her husband. Berenson insists that the jury therefore found her innocent "because she appeared to affirm the traditional conceptions of a gender order increasingly in doubt" (p. 243).

This contention can be sustained only by deleting pertinent material. Berenson never mentions that the defense argued Calmette's responsibility for his wounds because he dived to the floor in fear, Henriette Caillaux, according to this reconstruction, having meant simply to frighten him and thus fired low. The testimony of medical and forensic experts about this issue was heated. Berenson likewise ignores the effort of Joseph Caillaux's political allies to influence the trial by assigning to it a prosecutor and a presiding justice who owed their positions in the judicial hierarchy at least indirectly to him. He mutes the extraordinary character assassination of Calmette mounted by the defense and the degree to which Calmette's own attacks on Joseph Caillaux—above all for his handling of the Agadir negotiations with Germany and his involvement in the Henri Rochette financial scandal, both in 1911—were justified, even taking their political differences into account. So much of

the lore is missing from Berenson's version of the Caillaux affair that it is made to appear much less convoluted than it was. Finally, the number of factual errors is troubling, among them the circumstances of Joseph Caillaux's first election to the Chamber of Deputies, the details of his elevation to the Ministry of Finance, the reasons for Georges Clemenceau's long exclusion from cabinet office, the capacity and reputation of presiding justice Louis Albanel, and the extent of Radical Party involvement in the Rochette scandal. In spite of her acquittal, the evidence was heavily against Mme. Caillaux; the evidence is heavily against Berenson as well.

BENJAMIN F. MARTIN  
Louisiana State University

KENNETH MOURÉ. *Managing the franc Poincaré: Economic Understanding and Political Constraint in French Monetary Policy, 1928–1936*. (Studies in Monetary and Financial Policy.) New York: Cambridge University Press. 1991. Pp. xiv, 314. \$59.50.

Drawing on a wide variety of published and unpublished sources (including files of the Bank of France and the Bank of England, French and British treasury papers, and the private papers of Vincent Auriol, Paul Reynaud, Frederick Leith-Ross, Henry Morgenthau, and many others), Kenneth Mouré has put together a remarkably detailed and nuanced account of France's ill-fated effort to return to the gold standard between 1928 and 1936. Accepting the widely held diagnosis that overvaluation of the franc was the root cause of France's persistent economic difficulties after 1931, Mouré seeks to explain why the French clung so tenaciously to the gold-backed "franc Poincaré"—and adopted the severely deflationary policies that its defense required—in the face of mounting evidence that such policies precluded real economic recovery. In essence, he wants to determine why French officials acted less "rationally" in dealing with the Great Depression than their British, German, and American counterparts.

Mouré gives much attention to the public and private writings of the bank and treasury managers, politicians, and financial experts most responsible for formulating French monetary policy in the late 1920s and early 1930s. He tends to assume that these were public-spirited men whose only sin was faulty "economic understanding." And perhaps they were. Inasmuch, however, as France in these years had more than its share of shady operators looking for political favors and more than its share of financial scandals (of which the Stavisky affair was the most notorious), Mouré might have given more attention to the dark side of French public finance, including the role of currency speculation and what Jean-Noël Jeanneney calls "hidden money." A closer look at the private connections and interests of French decision makers

might reveal that policies that now seem to have been irrational for the long-term public good of France in the early 1930s were in fact quite rational for the short-term private good of those who made them.

Mouré also devotes considerable space to reconstructing all the twists and turns in international monetary relations and domestic policy formation between 1928 and 1936, from the Anglo-French negotiations on gold flows before 1931 (chap. 2), to the politics and diplomacy of the London Economic Conference of 1933 (chap. 3), to the tortuous process by which the franc was finally devalued in 1936 (chaps. 6 and 7). It is always salutary to be reminded that large policies are usually the cumulative product of many small decisions, but there is also a danger of not seeing the forest for the trees. And Mouré does point out a lot of trees. In any case, much of the diplomatic and political history that he recounts will be of interest mainly to the handful of scholars working on this topic. Of more general interest—at least to economic historians of modern France—are chapters 4 and 5, which provide an excellent explanation of how the Bank of France functioned in this era and how it interacted with the treasury in its efforts to manage the currency and maintain the solvency of the French state.

Ultimately, Mouré's search for the source of France's ill-conceived monetary policy takes him beyond the thought and actions of specific leaders and the details of diplomacy and politics to what might be termed France's "official mind" on the monetary issue: more specifically, the quasi-religious commitment to gold that emerged (or reemerged) in official circles in response to the monetary problems of the early 1920s. Once institutionalized in the franc Poincaré and seemingly validated by France's apparent immunity to the initial stages of the world Depression in 1929–30, this commitment prevented France from following Britain and America off the gold standard in 1931–33 and, indeed, locked it instead into the disastrous deflationary policies of 1931–35. Only when the Popular Front took power in 1936 did a change in French monetary policy become possible, and even then it was undertaken so reluctantly and in such a clumsy and ill-timed fashion as to virtually ensure continued economic crisis. Having learned the wrong economic lessons from its experience of the 1920s, France had to unlearn them the hard way.

Like most books that are in fact lightly revised doctoral dissertations, this one presumes extensive knowledge of the subject and is inevitably intended for a small scholarly audience. Nonetheless, it is an unusually well-researched work that sheds much light on a difficult subject. In the process, it eloquently announces the arrival of a talented young historian from whom we may expect more excellent work in the future.

MICHAEL S. SMITH  
University of South Carolina



CLAIRE ANDRIEU. *La banque sous l'occupation: Paradoxes de l'histoire d'une profession, 1936-1946*. Paris: Fondation Nationale des Sciences Politiques. 1990. Pp. 331.

Claire Andrieu's book is a great deal narrower than is suggested by the rather misleading title. It examines the turn to corporatist regulation during the Occupation and after the Liberation and explains why those bankers who had opposed regulation (which they described as the "socialization of credit") most vigorously in 1936 under the Popular Front later came to accept it. The main focus is on the banking laws of 1941 and 1945. The laws of June 13 and 14, 1941, created a "Professional Committee of Organization" that policed banking practice. In 1945 these laws were supplemented with the nationalization of half of French banking and the planning of industrial financing. Andrieu examines the involvement of a number of individuals during the political changes that took place between 1936 and 1945. He seeks to demonstrate the continuities of the professionalized banking elite and to tell the story of a change in the intellectual environment as ideas about the proper role of the state in finance and in the economy were transformed. Andrieu emphasizes that the new beliefs transcended the political cleavages of the period.

This book belongs to the history of elites rather than to economic history. There is almost no information about the actual role of the banks in the economy of the 1930s and 1940s. The policies of the banks themselves, and their relations with the German occupation authorities and with German banks, are not explored. Perhaps this is because Andrieu had little access to bank archives. But it is odd to have a history of bankers in which banking plays so small a part.

HAROLD JAMES  
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TEMMA KAPLAN. *Red City, Blue Period: Social Movements in Picasso's Barcelona*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xiv, 266. \$30.00.

Temma Kaplan's narrative brings together the people and events that constituted an extraordinary range of contests for urban expression and public domains in Barcelona between 1888 and 1939. Kaplan focuses on episodes that highlight the intersection of gender, class, and ritual in the evolution of a conflictive urban society. These diverse voices of city life and politics frame her perspective on the work of Pablo Picasso as a distillation of many popular currents as well as elite aesthetic debates within their complex economic, social, and political contexts, local and national. Both in its creative detail and its extensive dialogue with the work of European and American scholars, the book raises challenging questions for Barcelona's social and cultural history as well as more general directions for integrated urban studies

encompassing history, anthropology, geography, and gender studies.

After an overview of the urban symbolic landscape, Kaplan turns to the popular vocabularies available at the turn of the century to express criticism. In contrast to elite control of world's fairs (in 1888 and 1929) and many public institutions, she shows how religious celebrations, strikes, street life, bombings, and funerals distilled popular visions of—and actions on—the industrial city.

Complementing recent trends in Picasso scholarship as well as urban and cultural history, Kaplan shows that artistic developments could not be divorced from these social movements. Careful readings of Santiago Russinyol, Ramón Casas, and other artists involved in Catalan modernism show how such interests as puppetry, the rediscovery of religious and popular art, and the debates of Catalan nationalism participated in changing metropolitan culture.

In her choice of key ritual moments, Kaplan underscores the central role of women as active participants in urban life as organizers, information specialists, and participants in street actions. Moreover, she describes the symbolic status through which women also galvanized a critical consciousness into protests against repression, represented by the mass outpourings surrounding the funerals of young florists killed in police-promoted bombings in 1905. In reconstructing both active and representative roles, Kaplan underscores women as "dangerous" participants in urban life, epitomized in the dichotomy of nun and prostitute that was repeatedly played out in urban streets.

In the early twentieth century, the theaters of urban and national conflict became broader and more violent in major conflagrations, from the church burnings and civil unrest of the Tragic Week of 1909 through later strikes. The underpinnings of both assassinations and ritual protests expressed not only dynamic class relations but also crosscutting tensions between Catalonia and the Spanish state. Here, Kaplan's depictions blend salient individuals with more anonymous collective actions that participated in political and economic evolution.

The concluding chapter examines cultural reactions to the republic and Civil War. The analysis of a Picasso more distant from Barcelona life synthesizes the impact of both popular and elite culture evident in Kaplan's illuminating reading of *Guernica*. A rapid epilogue brings readers up to date on Catalan developments.

Such a broad overview inevitably faces some limits. The reliance on episodic foci creates an occasional imbalance between analytic depth and social continuities where some important contextual features seem reduced to an inactive backdrop. The reader familiar with Barcelona also may be disconcerted by English translations for Catalan topography, while those unfamiliar may construct a misleading cityscape from infelicitous translations such as Gràcia Pass (for



*Passeig de Gràcia*, where “boulevard” seems preferable). Maps integral to arguments are relegated to appendixes, although artistic and photographic representations are quite sensitively integrated into analyses. Indeed, the book proves compelling in its incorporation of the cityscape, both physical and human, into historical study as it illustrates the complex challenges of changing urban topographies and the neighborhood ethnography of class and violence demanded for future research.

Throughout the text, however, Kaplan situates these complicated problems within contemporary concerns of urbanism, art history, and women's studies as well as particular questions of Catalan scholarship. Thus, published at a time when Barcelona demanded world attention, this book provides an important, evocative vision of the processes that have formed—and continue to shape—the lives of that metropolis and its citizens.

GARY W. McDONOGH  
Bryn Mawr College

MARTHA A. ACKELSBURG. *Free Women of Spain: Anarchism and the Struggle for the Emancipation of Women*. Bloomington: Indiana University Press. 1991. Pp. xvi, 229. Cloth \$39.95, paper \$14.95.

When political or religious institutions are undergoing rapid change, questions about women and community frequently come to the fore. This happened when Christianity challenged the late Roman empire, when Quakers and Methodists confronted the established British church, when peoples in Africa, Asia, and Latin America resisted colonization, and in most of the great revolutions of the twentieth century, including the Spanish Civil War. Sometimes the women who participate in these kinds of movements do so without regard for their own particular goals, subordinating their needs as women to those proclaimed as the higher good. At other times, women demand rights for themselves as full members of society.

Martha A. Ackelsberg shows through oral history, judicious use of archival material, and comparative studies of past and contemporary protests, how the Spanish anarchist women's organization known as *Mujeres Libres* (Free Women) and the newspaper of the same name fought for improvement in the conditions of women as well as the liberation of their communities. As with much of the best history, this study is comparative and analytical, raising questions about anarchism by placing its development in the context of twentieth-century collective action.

The Spanish Civil War from 1936 to 1939 unleashed demands for the revolution anarchists had been expecting for generations. As one of their group contributions, *Mujeres Libres* promoted *capacitación*, the process by which the potential of ordinary women was discovered and then enhanced through educa-

tion and technical training. According to Ackelsberg, *Mujeres Libres* developed the talents of women, training them to assume leadership roles within anarchism as well as to pursue their own needs. *Mujeres Libres* spoke for a self-identified community in which women were equal members, but the movement as a whole did not necessarily share their views.

Although a communitarian focus with its promise of direct democratic control over social resources had enabled anarchism to flourish in Spain by attracting unlettered peasants, seamstresses, bakers, shoemakers, and factory workers, to name only a few, the core leadership during the Civil War came from the National Confederation of Labor (CNT). Its perspective, as with Wobblies in the United States, was that of male workers in heavy industry. The CNT wanted *Mujeres Libres* to collapse its activities into committees that men dominated. Refusing to become a woman's bureau of the CNT by subordinating the special needs of women into a universal struggle for emancipation that men would define, the organization maintained its autonomy, paying a price for its independence by being denied access to the congresses where policy was determined.

Loyal as it was to anarchism's general goals, *Mujeres Libres* also expressed the particular experiences of working-class wives and daughters, workers, physicians, and intellectuals. From their perspectives, the movement could not afford to wait until the war was over to transform society by attending to child care, women's health, and education, since the revolution had to be brought into existence during the course of struggle. Once *capacitación* had empowered women to recognize their interests and to act on them, it could not be stymied.

It is in the context of differences among members of the same movement that the book will be of most interest to scholars of contemporary women's history. Bringing theoretical concerns very much into the present, Ackelsberg makes a plea for recognition of diversity and of the possibility for activists to hold many identities while pursuing social change. When historians take women's movements and gender differences in mixed organizations—as distinct from feminist institutions—seriously, this book will become part of the canon. Even now, it represents a forthright effort to view women's participation in politics in exciting new ways.

TEMMA KAPLAN  
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HELEN GRAHAM. *Socialism and War: The Spanish Socialist Party in Power and Crisis, 1936–1939*. New York: Cambridge University Press. 1991. Pp. xii, 327.

Spain's Socialist Party (PSOE) was a key participant in the politics of the Second Republic, headed two civil war governments under Largo Caballero and Juan

Negrín from 1937 to 1939, and was confined to a bitter and powerless exile during the Franco years. Given its long history as a moderate, social democratic party, the PSOE was surely expected to be a pillar of the republic and a fighter for democracy against the multiple forces of the Right. Yet its actions, especially after 1934, offer a challenge to historical interpretation. It fought itself, with only division and hostility as the result; boisterous rhetoric, from its left wing alienated would-be allies, and it had no counterpart in action; and its leaders were strangely oblivious to danger, resulting in momentous miscalculations.

Helen Graham offers an account, based extensively on PSOE documents recovered from Moscow, and an interpretation of this sad history. In it there are no heroes, no enduring victories, no end to missed opportunities, and no relief from intraparty rivalries, even as the enemy pursued its mission of destruction. This is principally a study of leaders for whom tactics and position, not ideology, were paramount; their words were in the service of politics, not of ideas. Graham is severe in her judgments, often repeated, of the party's two major political gladiators. Caballero indulged in rich revolutionary rhetoric at odds with his reformist practice, and his leadership as prime minister in 1936–37 was "unequivocally and unalterably reformist" (p. 89), that is, hostile to the popular revolution that erupted with the uprising of the military. Indalecio Prieto, once a statesman of the republic, lacked foresight, dreaded his own failure, refused his responsibilities, and sank even deeper into pessimism and passivity; "bitterness sapped his energy and distorted his political sensibilities" (p. 133). The quarrel between the two leaders, which divided Socialists and dissipated their strengths, has been cited frequently as one cause of the Spanish tragedy, and Graham's work reaffirms that view.

Yet the failure of the PSOE should not be attributed to personalities alone. Graham understands this well, even as her focus is fixed on its leadership. She dismisses well-known "conspiracy theories" (p. 5) that allege that the Communist Party and its Soviet benefactors, by subversion and ruthlessness, were the reasons for Socialist eclipse. She is skeptical that Caballero and his followers experienced anything close to an ideological conversion in embracing revolution in 1934 in response to the accumulated disappointments of collaboration with Republican politicians. The matter was not merely one more chapter in the timeless quarrel among Socialists who demanded that a choice be made between reform and revolution. Anarchist pressure, militancy, and the increasing appeal of their union, the Confederación Nacional del Trabajo (CNT), had their effects on Caballero and the Socialist union, the Unión General de Trabajadores (UGT), as did the climate of violence from 1934 to 1936. So too did heightened militancy from the party's youth organization and rank and file, notably in the south, which was possibly the main reason for the *Caballeristas* to lurch leftward, if only in

rhetoric and principally as a means to control the PSOE. So an internal struggle for power, pitting the Left against those who sought alliance with bourgeois politicians, predated the war. The war, Graham argues, exacerbated the struggle, which became a private civil war in the midst of a civil war.

This is an important work, the product of exemplary research. If there is fault, it lies in the tone and the focus of the study on the leaders of the PSOE. Graham appears to believe herself wiser than those whom she studies; hence the running criticism of their actions. And much remains to be done in study of the party, its organization, militants, and voters.

NATHANAEL GREENE  
Wesleyan University

A. TH. VAN DEURSEN. *Plain Lives in a Golden Age: Popular Culture, Religion, and Society in Seventeenth-Century Holland*. Translated by MAARTEN ULTEE. New York: Cambridge University Press. 1991. Pp. viii, 408. \$59.50.

Historians of the Dutch Republic have long been indebted to A. Th. van Deursen for his four-volume, eminently useful *Het kopersgeld van de Gouden Eeuw* (1978–82). Happily, this study of "ordinary life" in the province of Holland between about 1570 and 1650 has now been made available in English, conveniently packaged—in ironic contrast to the historically appropriate chapbook format of the original—into a single, large, elegantly produced volume.

Van Deursen's avowed goal is a modest one: to describe, rather than to "analyse or explore in depth," how "the common people of Holland lived during the war with Spain" (p. ix). He readily admits that he cannot hope to portray every facet of life among the "non-elite," but the range of topics is considerable. Part 1, "Daily Bread," includes not only the obvious themes of work, wages, and prices, but also beggary, the limited possibilities of socioeconomic mobility, and resentment against a myriad of immigrants. Van Deursen shows us a less than ideal picture of economic conditions in famously prosperous Holland; although likely "better off" than their counterparts elsewhere, the majority of common people in urban Holland struggled to get by, so that a young family of undistinguished background probably could not have lived on the earnings of only one adult. In part 2, "Popular Culture," he addresses a similarly wide range of subjects. Individual men and women, as well as men and women as groups, are discussed in various contexts throughout the book, but this section includes specific chapters on perceptions and roles of each gender. Van Deursen then deftly moves to education, the spread of news, and recreational reading.

"People and Government," the theme of part 3, is a good example of how the author avoids the temptation to stray from his theme: the topic is not Holland's

government per se, but rather how that government was experienced by common burghers. Holland's rulers wanted to be obeyed as much as rulers anywhere, but lacking the potential force of a Henri IV or Richelieu, they soon discovered that the key to power and order was a lot of winking and ignoring—the result for ordinary people was a relatively free existence. Of greatest interest and use may be part 4, "Hell and Heaven." Van Deursen sympathetically and ably treats the three most important religious groups in the province (Calvinists, Catholics, and Mennonites) in separate chapters, but perhaps the best chapter is the first, in which he combines elements of the three religions into the theme of "popular beliefs." Of interest for nonspecialists might be the chapter on Catholicism, which synthesizes the reasons why neither Holland nor the republic as a whole were convincingly Calvinist.

Van Deursen certainly succeeds in his stated attempt to provide information and a rather static, descriptive picture. The book is full of colorful anecdotes and characters, useful statistical tables, and well-chosen illuminations. But he also does more than "describe." The choices he has made from an array of potential sources and topics, and his command and thoughtful treatment of sources and events, are exercises of analysis, as he undoubtedly knows, and they are masterful enough to cause envy. Although printed and secondary works constitute the foundation of the study, there is—for a work that masquerades as a general survey—a surprising number of manuscript sources as well.

He notes the weakness of the book himself: his description of each topic, good as it is and based on a single province, is still quite general. More focused investigations into local archives will illuminate a clearer, deeper picture of "all the subjects we have investigated" (p. 320). At the same time, the study—and all future research into each topic—would benefit from placing the particulars in Holland into a wider context. He states from the beginning that conditions in this largest province were "not necessarily" typical throughout the republic, but more explicit comparisons with other provinces and urban areas of Europe would help to make Holland's situation more significant (some helpful contrasts are drawn, but most secondary materials are from Dutch historical literature). Still, the problems here are not serious enough to detract from the importance and utility of this book.

I am not sure of the translator's first language, but if it is Dutch, as I suspect it is, then his rendition is a remarkable achievement, as anyone who has attempted to translate into a non-native tongue knows. Having said that, I must add that to my ear the English too often rings untrue. Nevertheless, this is a volume that should be of great interest and use to any historian of early modern Europe, despite its modest

goals, and despite the number of years elapsed since its original publication.

CRAIG HARLINE

Brigham Young University

MAARTEN KUITENBROUWER. *The Netherlands and the Rise of Modern Imperialism: Colonies and Foreign Policy, 1870–1902*. New York: Berg; distributed by St. Martin's. 1991. Pp. vii, 407. \$76.50.

Dutch historians have traditionally assumed that the Netherlands in the latter part of the nineteenth century did not engage in a policy of imperialism. While other powers carved out or enlarged their empires, the Netherlands merely established firmer control over its existing colonial possessions. This contrast with other nations was further reinforced, according to Maarten Kuitenbrouwer, by the introduction of the so-called "ethical policy" into the East Indies by which the Dutch allegedly were bringing order, welfare, and civilization to the native population. Kuitenbrouwer refutes this widely held view and tries to demonstrate that the Netherlands, beginning in the 1870s, did pursue imperialistic policies in Asia and even in Africa. Such is the principal thesis of this unabridged translation of his doctoral dissertation, which is based on a vast amount of printed and archival materials.

According to Kuitenbrouwer, late-nineteenth-century Dutch imperialism in Asia began with the conquest of the Sultanate of Acheh in North Sumatra. Although in the 1820s the Netherlands had recognized the independence of the Sultanate, in 1873 it initiated hostilities against this Indonesian state and embarked on a long and bloody war of conquest that finally ended in 1903, and was followed by further "pacification" over the next ten years. The war against Acheh was costly: some 60,000 to 70,000 Achinese and 2,000 members of the Dutch colonial army were killed. In addition, 10,500 colonial troops, 25,000 Javanese forced laborers, and an unknown number of Achinese died of other causes. Yet as Kuitenbrouwer amply demonstrates, this military venture had widespread Dutch political and popular support. At about the same time the Netherlands established administrative control over other parts of its Asian empire.

In the latter part of the nineteenth century the Dutch also became interested in the two Boer republics, Transvaal and the Orange Free State. Their struggle against Britain had significant popular but not government support. Like the Acheh War, much of the pro-Boer sentiment was an expression of Dutch nationalism, which eagerly embraced the "rediscovered" long lost kinfolk as a reinforcement of Dutch presence in the two South African republics. Moreover, many considered South Africa as possible colonization territory for the Netherlands in the event it would lose its independence in Europe. The

Dutch invested in the economic development of the Boer states and many Netherlands were active in education and the civil service. During the war of 1899–1902, Dutch public opinion strongly supported the Boers and denounced Britain. But the Dutch government refused to become embroiled; it realized how much it depended on Britain for its imperialistic endeavors in Asia.

Although Kuitenbrouwer argues rather persuasively and buttresses his argument with an impressive amount of detail, much of his thesis rests on shaky ground. Is it valid to contend that the Dutch conquest of Aceh was a typically nineteenth-century imperialistic venture? Could one not convincingly argue that in 1873, what is now known as Indonesia was generally considered part of the Dutch colonial empire or at least a Dutch sphere of influence? Moreover, one might argue that Dutch interest in South Africa did not smack of imperialism but expressed a genuine interest in forgotten kinfolk. This is nevertheless a solid piece of scholarship that will stimulate much discussion and further research.

GERLOF D. HOMAN  
Illinois State University

EVA LABOUVIE. *Zauberei und Hexenwerk: Ländlicher Hexenglaube in der frühen Neuzeit*. Frankfurt a.M.: Fischer Taschenbuch. 1991. Pp. 302. DM 19.80.

German contributions to recent witchcraft literature range from wretched to superb, but Eva Labouvie's book clearly belongs near the upper end of the scale. It focuses unwaveringly on the rural and popular contexts of witchcraft in today's Saarland. During the early modern period this small region contained 550 villages and eleven small towns, divided among four states (two Catholic, the Archbishopric of Trier and the Duchy of Lorraine; and two Protestant, the County of Nassau-Saarbrücken and the Duchy of Zweibrücken) with two condominiums. Witch-hunting was endemic throughout the Saarland, reaching a total of 591 trials and 467 known deaths, mostly in its Catholic zones; nearly all of them took place during the sixty years after 1570, with a scattering of earlier cases and another handful of isolated trials between 1679 and 1700. Relative to population, witch-hunting in the Saarland was twice as intense as in Baden-Württemberg and eight times as severe as in Bavaria (p. 68).

None of the Saarland's absentee rulers exercised close control over witch trials in this unimportant and impoverished backwoods. Labouvie presents much evidence demonstrating that Saarland witch-hunting originated almost exclusively from beneath, with ad hoc village committees (*Ausschüsse*) responsible for organizing and managing most trials. Relatively passive overlords, most of whom had no financial interest in witch-hunting, cooperated with them: fewer than two dozen Saarland witch trials ended with libera-

tions. Lorraine, the only Saarland state to confiscate the property of convicted witches, had insisted since 1569 that all death sentences against witches be confirmed by the ducal government, thus avoiding *Ausschüsse*. Elsewhere these "village Inquisitions" (Labouvie's term) appeared with the first wave of prosecutions in the 1580s and remained extremely busy until the Thirty Years' War abruptly ended their activities in 1634 by severely depopulating the entire region.

Witch-hunting was thus an extremely localized enterprise in the Saarland. Labouvie traces in meticulous detail the ways in which these rural communities partially assimilated official demonology, twisting their image of the spell-casting *Schadenzauber* toward the Devil-worshipping *Hexe*. She even pinpoints a generation gap among witnesses around 1590, with the under-thirty population willing to describe elements of the witches' Sabbath (pp. 194–95). But demonology never fully triumphed at the village level, where Saarlanders continued to assimilate Sabbaths to local festivals; witches "held a Kermis" (p. 229), while male witches became "black pipers" (*zaubersen pfeiffer*) before 1600 and "witch-pipers" (*Hexenpfeiffer*) after 1650 (p. 77).

Labouvie's investigation also exploits an unusual source: the 182 defamation suits brought by Saarlanders who had been publicly insulted as *Schadenzauber* or, after 1650, as *Hexen* or even werewolves (pp. 74 and following). Before 1587 and after 1634 such lawsuits overwhelmingly (85 percent) involved women, although during the apex of witch-hunting most such plaintiffs were men. Defamation suits were rarely brought during witch hunts, however, since would-be plaintiffs were usually helpless against the local *Ausschüsse*.

Excellent and innovative in its microscopic rural dimensions, Labouvie's work occasionally stumbles when she gets too far away from her district. Lamentably, outdated misinformation such as the Toulouse "Sabbats" of 1353 (p. 22), the assassination in 1485 of a Spanish Inquisitor by the brother of an executed witch (p. 30), or Mandrou's misinterpretation of French witch trials as drastic and severe (p. 37) mars her introductory section. But once she enters the Saarland, Labouvie becomes a sure-footed guide who rewards her readers with several fresh insights about an old topic.

WILLIAM MONTER  
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ANN TAYLOR ALLEN. *Feminism and Motherhood in Germany, 1800–1914*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. x, 299. \$42.00.

Ann Taylor Allen takes us on a mission of historical rescue. Her study addresses the pervasive but poorly documented charge that "maternalist" German feminism of the nineteenth century contributed to later



Nazi family and reproductive policy. Her strategy is to return to the intellectual, rhetorical, and political context in which "motherhood" was discussed in nineteenth-century Germany to show the richness and political diversity of the metaphor. Her focus is thus on political language, its development, its nuances, and its alteration as it traveled through time and across the political spectrum. She has also chosen a biographical approach; her study centers on the lives of influential and representative groups of women whose words and activities embody the connections between motherhood and feminism. The use of biographical materials enriches her analysis but also leads to some problems. The effort to rescue the reputations of unfairly castigated women tends toward heroine-building, no doubt a risk implicit in the biographical method. For the most part, however, the mission succeeds. Not only will readers be persuaded of Allen's critique of the view that theorists of motherhood were proto-fascists; they will be provoked into rethinking many aspects of German history and feminist theory in the process.

The first part of the book focuses on the kindergarten movement of the early and mid-nineteenth century. Through the work of the Froebel Society and affiliated schools for young children, German women could find a rationale and opportunity for female roles that escaped subservience to fathers and husbands. "Motherhood" was at the core of this alternative notion of female identity; it was the basis for extending female responsibilities into the world outside the home as well—the "great social household." Even in these early discussions, Allen argues, motherhood, far from taking on an essentialist or biological emphasis, was envisioned as a social function.

The heart of the argument rests on the biographical analysis of six exemplary women involved in the kindergarten and "spiritual motherhood" movements. Their personal lives reveal a variety of strategies for building from the base of "motherhood" (significantly, only three of these women were either biological mothers or stepmothers). Most had a history of personal rebellion against the constraints that they experienced because of domination by fathers or husbands, but whatever their family status, they used motherhood metaphors to justify their activism.

The choice of focus is a bit problematic here. Restricting the detailed study to women close to the kindergarten movement who might be expected to emphasize maternalist rather than other grounds for self-legitimization means that claims about the centrality of "motherhood" to the emergence of feminist critiques remains undemonstrated. But among these women, at least, the power of the language of motherhood becomes clear. The second problem—that of rescuing by overstatement—surfaces occasionally in this section as well. One example appears in the quite interesting and generally subtle argument about the sense of "social mission" and interest in class recon-

ciliation apparent in the activities of many "spiritual mothers." Allen generally recognizes the unconscious social prejudices of her biographical subjects, but occasionally she is too bent on rescue to notice tensions. She states, for example, that Henrietta Goldschmidt "criticized the snobbery of leisured women who looked down on their wage-earning sisters" only to demonstrate this by citing Goldschmidt's efforts to improve the social status of governesses: "The Froebel Society seeks for its graduates a position in the family which *does not place them in the same category as servants*" (pp. 98–99, my emphasis). Apparently not all wage-earning sisters were equally deserving of sisterly solidarity.

The second half of the book indeed departs from the smaller circle of the kindergarten movement to look at women who were engaged in a wide range of activities aimed at improving the lives of mothers and children in late imperial Germany. Here, Allen demonstrates the many ways in which women could use maternalist metaphors and arguments to support a variety of political activities. Her analysis turns most convincingly on the reconstruction of exemplary careers such as that of Frieda Dünsing, who used her legal education to call attention to and address men's abuse of women and children. Allen is certainly straightforward in recognizing that the paradigm of motherhood was put to more conservative uses as well, although here again the biographical choice tends to deemphasize this reading. Still, her evidence certainly is persuasive on the point that not everyone who spoke on behalf of motherhood (and even, more controversially, who entertained eugenicists' arguments) was somehow a precursor of fascism. The discussion of the strain of eugenic thought that intertwined with maternalist arguments around the turn of the century will certainly be the most controversial aspect of Allen's argument, but she is to be commended for taking on the crucial task of setting these questions simultaneously in their immediate context and in the framework of long-term historical development. On this controversial issue, as throughout the study, Allen's engaging presentation of her rich array of historical data, her attention to context, and her engagement with both broad historical issues as well as comparative and theoretical questions (the latter especially striking in the very thoughtful concluding chapter) make this a satisfying model of new intellectual history.

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SONJA-MARIA BAUER. *Die Verfassungsgebende Versammlung in der Badischen Revolution von 1849: Darstellung und Dokumentation.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 94.) Düsseldorf: Droste. 1991. Pp. 377. DM 78.



The Grand Duchy of Baden holds a unique place in the annals of Germans' repeatedly aborted efforts to implement liberal and democratic ideals. It was in 1849 that Baden—already acknowledged to be Germany's most liberal state—became the site of nineteenth-century Germany's only revolutionary upheaval in which the revolutionaries were successful in taking over the entire apparatus of state. For a mere seventy days, Baden was ruled not by a monarch but by a democratically elected parliament and a three-person executive appointed by and accountable to that parliament. Despite the great symbolic importance that this phenomenon holds—given the trajectory of German history—the Baden revolutionaries' parliamentary activity had never been reconstructed and evaluated in adequate detail. This is the task Sonja-Maria Bauer set herself. The first half of the book consists of an analysis of the primary documents—protocols of the parliament's sessions, committee reports, and individual members' initiatives—that constitute the second half. Biographical material and helpful charts are also included.

Bauer's main contributions are twofold. First, through a careful analysis of the election process and results, Bauer shows that popular support for the revolutionaries was more substantial than many previously thought, as high as 60 percent of the adult male population in some areas. Second, Bauer challenges those critics of the ruling revolutionaries who suggest that the parliament was politically ineffectual; she calls attention to the profusion of issues the embattled parliament addressed in its short life, the remarkably democratic manner in which it did so, and the political innovations it introduced (especially the right of the parliament to initiate legislation, as well as to interpellate and even to depose the executive). Although also documenting tensions among the revolutionaries, their inadequate attention to the "social question," and their various financial and military dilemmas, Bauer concludes that the revolutionaries' experiment was an important moment in the development of parliamentarism and party politics in Germany that should be taken seriously on its own terms despite its untimely defeat at the hands of Prussian and Hessian troops.

Much of the first half of the book is given over to restating the contents of the second half. Interesting issues Bauer touches on that could have been probed in greater depth include the tensions between bringing about economic justice and needing the population's money to finance the revolution's military defense, between the grindingly slow process of parliamentary democracy and the urge to make swift and fundamental political changes, between tolerating and silencing ideological opponents, between maintaining administrative stability (thus being dependent on the bureaucrats of the previous regime) or accepting chaos, and—in the search for persuasive symbolic imagery—between building on and destroying the past. A fuller engagement with these matters

would have made the book into more than a contribution to the history of parliamentarism in Germany. It would have contributed substantially to our understanding of the persistent dilemmas of democracy in the modern world.

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ANDREAS KAERNBACH. *Bismarcks Konzepte zur Reform des Deutschen Bundes: Zur Kontinuität der Politik Bismarcks und Preussens in der Deutschen Frage*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 41.) Göttingen: Vandenhoeck und Ruprecht. 1991. Pp. 266. DM 68.

The subject of this Bonn dissertation—Otto von Bismarck's plans for reform of the German Confederation—is a well-traveled road. What new perspective has Andreas Kaernbach to offer? In the preface he states that he has reexamined earlier interpretations on the basis of "previously unknown archival sources." Since the early years of Bismarck's career and the period of unification are the best documented of his career in multivolume publications long in print, this announcement is enough to arouse the interest of any historian of the period. From state archives in Copenhagen, Berlin-Dahlem, Bonn, Merseburg, Koblenz, and Munich the author has gleaned a few relevant documents, which, although often interesting in themselves, are not vital to his thesis. On the contrary, he develops his essential argument from familiar sources long available—Bismarck's collected works, the massive editions of documents on Prussian and Austrian foreign policy, some additional documentary collections, numerous memoirs, and the like.

Kaernbach's thesis is that Bismarck's objective was always a limited one of nonrevolutionary character, not very different from those of other Prussian foreign ministers as far back as 1849 or even 1807. Up to the revolution of 1848, Austria and Prussia had cooperated in dominating the German Confederation. But Metternich's successors abandoned his practice of reaching agreement with Berlin before undertaking any noteworthy action in the Diet of the German Confederation. During the 1850s they preferred to cooperate with the German lesser states against Berlin. Prussia's response was to seek a reform of the Confederation that would have recognized the parity of the dual powers, including the mutual right of veto in the Diet. Even before Bismarck became minister president of Prussia in 1862 his predecessors had demanded joint command of confederate armies in wartime and hegemonical control over northern Germany. These gains would have merely recognized the reality of Prussia's ascending economic, military, and political importance relative to Austria in central Europe.

According to Kaernbach, the obstinate refusal by

Austrian statesmen from Felix von Schwarzenberg to Johann Bernhard von Rechberg to grant parity with Prussia compelled Bismarck to become a "white revolutionary." By yielding, they could have retained southern Germany as an Austrian sphere of influence, preserved the constitutional link with Germany as a whole, and prevented the appearance of the German Reich of 1871. Bismarck, he implies, would have remained satisfied with this result, and Germany and Europe would have been better off for it.

This thesis will not find favor among historians bent on rehabilitating the performance records of Austrian statesmen from Schwarzenberg to Rechberg. Neither will it please neo-Marxist historians, to whom the increasing weight of material progress was decisive for Prussia's diplomatic and military triumph over Austria. In the opening chapter Kaernbach quotes approvingly passages from Leopold von Ranke (also Klaus Hildebrand and Andreas Hillgruber) defending the influence of personality on the course of history and promises to illuminate the "complex personality of Bismarck." But he does little to fulfill that commitment. Had he done so, Kaernbach might have become less certain about the modesty of Bismarck's objectives in foreign policy. Bismarck was a highly narcissistic personality type incapable of accepting a secondary political role either for the state he served or for himself as its minister.

Although his analysis adds some ingredients to Bismarck's recipe for reform of the German Confederation, Kaernbach's concentration on that theme limits his understanding of the subject. Too little attention is paid to the dynamics of the European balance of power in the post-Crimean War period. Bismarck's antenna was closely tuned to shifting power relationships, and his strategy was designed to accommodate and exploit them—to enable Prussia to escape their dangers and penetrate their weaknesses. Prussia's position as the weakest of five great powers required a flexible strategy of alternative pressures and choices. In Bismarck's own words Berlin had to "hold open every door and every turning." Success in the quest for expansion in territory, population, and power depended on that strategy's effectiveness.

What Kaernbach describes as Bismarck's true goal (hegemonical control of northern Germany) was just one possible outcome—the minimal one. When Bismarck began (after 1851) to view German public opinion as a component in the system of pressures for the coercion of Austria and to refer (after 1858) to a German parliament as a Prussian objective in reforming the German Confederation, he created a maximal option of whose revolutionary import he was quite aware. Bluffing was not his way; threats had to be real possibilities, options to be carried out if circumstances permitted and wisdom dictated. "Many paths led to my goal," he told historian Heinrich Friedjung. "I had to try all of them one after the other, the most dangerous at the end. It was not my way to be

single-handed in political action." The narrow focus of Kaernbach's investigation obscures the full range of Bismarck's calculations in the 1860s.

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KONRAD H. JARAUSCH. *The Unfree Professions: German Lawyers, Teachers, and Engineers, 1900–1950*. New York: Oxford University Press. 1990. Pp. xiv, 352. \$55.00.

Konrad H. Jarausch's examination of three categories of German professionals is among other things an exercise in filial piety. Dedicating the book to his father and uncle, conservative secondary school teachers in Prussia during the Weimar and Hitler years, Jarausch draws occasionally on their experience, as well as his mother's (also a pedagogue), in confronting democracy and totalitarianism. His family's responses to Germany's drastic changes in regime, from his Uncle Bruno's skepticism toward the Republic and its Constitution Day to the wartime exhaustion, bewilderment, but finally optimism of Konrad Jarausch, Sr., were to a degree symptomatic of the *Bildungsbürgertum* in general (pp. 68, 132, 175–76).

Proceeding from this inspiration the author chose to investigate the professions of law (practicing attorneys), education (more precisely, classically trained philologists), and engineering. Their development since national unification, he argues, was sufficiently similar yet distinct enough to demonstrate both the common pattern and particular characteristics of a specifically German process of professionalization. Thus, all three occupations shared a potentially dangerous dependence on governmental authority for training, certification, and, not least, economic well-being. Their prosperity was decisively influenced by the number of candidates admitted to the appropriate faculties of universities exclusively controlled by the state, as well as the state's hiring, fee structure, and other policies. This situation exposed teachers, who were mostly civil servants, lawyers, and technicians, even with their ideal of freedom of choice (of clients, employers, and so on), to the pressures of politics. Jarausch shows that their reaction was invariably to use whatever autonomy they retained to defend a narrowly conceived self-interest, which meant as far as possible barring women and, after 1933, removing Jewish colleagues from their ranks. The "free" professions therefore were deprived of, or voluntarily abandoned, that essential quality until their restoration and partial rehabilitation under the liberal Federal Republic.

This systematically organized study begins with an analysis of the emergence of the professions under the *Kaiserreich*, but it concentrates mainly on the multifaceted crisis professionals faced in the 1920s and especially following January 30, 1933. Along the

often troubled path professionals trod from the Wilhelmine to the Adenauer eras (the immediate post-1945 period is only summarily treated as an epilogue), the author points out a series of crucial signposts marking professionals' collective decline "from internationally respected experts to accessories to Nazi crimes" (p. vii). One was their failure either to cope with the disruptions following World War I, for instance in the unprecedented numbers of would-be entrants into the professions, or to establish a positive relationship toward democracy during the 1918-19 revolution despite such promising innovations as the creation of several "councils of intellectual workers." The resulting mistrust of a parliamentarism that was unwilling to satisfy the demands of the professionals (for example, underpaid but elitist and misogynist teachers seeking respectable wages) turned into massive alienation under the impact of the Great Depression, with its constantly mounting unemployment and deeply resented public sector salary cuts. The author calculates that, surprisingly, only small minorities of mostly younger or marginal practitioners in each profession became early Nazi activists; however, they usually sufficed to accelerate the external and internal *Gleichschaltung* of their occupational organizations. Thanks to rearmament and war, engineers as a rule materially benefited from the Third Reich; all German professionals nevertheless were progressively demoralized by Hitlerism's pervasive racism, ideological fixations, lawlessness, and, in the end, crushing military defeat.

Notwithstanding a few misleading translations, factual errors, questionable assertions (was Hitler's personality really "colorful" [p. 116]?), and an excessive fondness for exclamatory sentences, this is a widely researched and persuasively argued treatment of a significant element in the sociopolitical make-up of modern Germany.

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JILL LLOYD. *German Expressionism: Primitivism and Modernity*. New Haven: Yale University Press. 1991. Pp. x, 267; 285 plates. \$60.00.

Early twentieth-century German art has been haunted by the fact that it was overtly entangled in political ideology. This politicization, combined with the emergence early in this century of an art history dominated by formalist analysis, has meant that German forms of modernism have been condemned at home and ignored abroad. One outcome of this has been the overevaluation of two groups, the *Brücke* and the *Blaue Reiter*, whose art has been perceived to fit into the French formalist model.

Against this background, Jill Lloyd has undertaken a revisionist project that is determined to set the German Expressionists within the social milieu of industrial modernization in the last decade of impe-

rial Germany. Focusing on the role of primitivism in the creation of modernist art, she argues that it was not primarily an artistic style copied from native artifacts but a manifestation of an antibourgeois bohemian way of life that was simultaneously grounded in bourgeois colonialism. It constituted a modernism that broke radically with the past and was part of the larger conservative antimodern movement of prewar Germany.

To demonstrate this multifaceted nature of primitivism, Lloyd selects four of the *Brücke* artists for extensive analysis. She documents the fascination with exotic and primitive cultures in Germany by drawing on illustrated newspapers, satirical journals, popular entertainment, return-to-nature fads, and scholarly essays. Stressing the enthusiastic but complicated responses of the artists to these phenomena, Lloyd provides lengthy discussions of the variations on primitivism from Erich Heckel's depictions of nude bathers and Ernst Ludwig Kirchner's street scenes to the ambiguous primitivism of Emil Nolde and Hermann May Pechstein that resulted from trips to the German South Sea colonies.

Lloyd's argument that primitivism grew out of the "living fabric" (p. 42) of the *Brücke* artists' lives and provided the means for them to negotiate between the past and the future in a complex modernizing society is convincing. She also, and I think correctly, makes her case that they were, in the final analysis, not social and political progressives, but "marked by the imperialist consciousness of their times, despite their rebellious challenge to bourgeois mores and values" (p. 31).

The very complexity of Lloyd's argument, however, often traps her in internal contradictions. Although she indicts the *Brücke* for its ultimate reaffirmation of the values of bourgeois culture, her whole approach continues the glorification of these artists and their bohemian activities. This is particularly marked in her analysis of the central primitivist image: the female nude. Here, although Lloyd acknowledges that *Brücke* artists tended to reconfirm bourgeois attitudes toward women, her lengthy analyses celebrate the "vitalist, liberated" sexuality—in which females are repeatedly treated as objects—that marked the studio life and art. She seems unaware of current studies about sexuality and power or of the problematic aspect of regarding female bodies as acceptable ciphers on which to inscribe modernity. Lloyd also continues the tradition of treating the Expressionists as if they were brilliant stars in an artistic vacuum in which all other artists in Germany were reactionary conservatives. She fails to recognize, for example, that the principles on which the *Brücke* was organized were not original, but were common to other contemporary German artists' groups.

Her revisionist drive, sadly, includes gratuitous attacks on many of the scholars she cites. This lack of generosity is compounded by an unusually large number of typographical errors and questionable

citations. On the positive side, the book contains a large number of illustrations, many in full color, and provides cultural information derived from German sources and archives not easily accessible to American historians. Although the attempt to integrate artistic creativity into the social historical mainstream is praiseworthy, this work also demonstrates the difficulties of that project.

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DIETRICH ORLOW. *Weimar Prussia 1925–1933: The Illusion of Strength*. Pittsburgh: University of Pittsburgh Press. 1991. Pp. ix, 368. \$49.95.

A paradox lies at the heart of Dietrich Orlow's deft analysis of Weimar Prussia: the nineteenth-century ethic of service to the state that encouraged responsible coalition politics among democrats, Catholics, and moderate nationalists also led to a calculated policy of appeasement toward increasingly authoritarian Reich cabinets, which eventually destroyed parliamentary rule altogether. Orlow introduces a Prussia run by able, reform-minded politicians whose achievements contrast with the conventional picture of Weimar Germany slipping into despotism. Over the course of the 1920s, left-leaning coalitions assembled by the Social Democratic Prime Minister Otto Braun eliminated vestiges of the state's feudal past and built an efficient and socially heterogeneous administration. The interconfessional peace that followed Prussia's 1929 concordat with the Vatican ranks among Braun's finest accomplishments.

On other questions, Prussia's legacy was more ambivalent. Braun blocked much-needed *Reichsreform* in order to preserve republican administration in the three-fifths of Germany under Prussian jurisdiction. Opposition served the short-term interests of the republic but lent credibility to right-wing arguments that considered national ends badly served by the democratic process. Braun also supported, however reluctantly, Weimar's unhappy string of neoconservative chancellors after 1930; to lead Prussia in a confrontation with the Reich was to risk the one pillar of democracy in Germany. This axiom may have kept Prussia's coalition partners constructively pragmatic in the late 1920s, but it made them too timid by the early 1930s.

The experience of the Great Depression makes clear how the financial vulnerability of the *Länder* enhanced the power of the federal government, because it was the Reich that subsidized state deficits. If disastrous economics weakened Prussian clout, unsettled politics justified federal encroachment. Carl Schmitt neatly summarized the balance of power in 1932: "whoever controls emergency legislation, controls the state." Orlow discusses at length the alterna-

tives available after Papen's coup on July 20, 1932. Unfortunately, he does not converse a great deal with fellow historians, missing entirely the early analysis by Earl Beck (*The Death of the Prussian Republic* [1959]) and the wonderful study of Berlin's police force by Hsi-huey Liang (*The Berlin Police Force in the Weimar Republic* [1970]).

Orlow's assessment of the coup is persuasive nonetheless: although there was little chance for broad protest in the name of the republic to succeed, more partisan action might have galvanized an antifascist opposition. In a brilliant section, Orlow steers the debate back to mentalités: "All Prussian political leaders, regardless of their ideology, were convinced that Prussia's primary historic service to Germany had been to create a unified nation . . . [F]aced with a choice between national unity and defense of a political system, [they] chose unity." Resistance to the Reich would have been a violation of their political credo (p. 241). The coup thus aptly illustrates the paradox of Prussia's democratic mission.

Orlow masterfully explains the divergence of Prussia and the Reich and delineates the diminishing options the Braun cabinet faced in the 1930s. But readers will miss a discussion about the idea of Prussia in German history, a topic that has been the focus of a number of fine essays in recent years. A more serious problem is his inattention to popular politics. The reader follows Orlow's account of the end of Prussia with a lingering sense that the real drama lies elsewhere.

The fact is that Prussian voters did not share the political pragmatism of Prussian leaders. Stability was maintained in the face of growing disgust with the Weimar system. Orlow did not intend to write a book about voters, but if he makes responsibility the key to Prussia's success story and thus condemns the People's Party for harboring "irresponsible demagogues" (p. 21), then it is worth noting the frantic anti-Marxism of voters. Would more responsible statesmanship in 1929 or 1930 really have changed the basic attitudes of the electorate? Orlow himself contends that Weimar never won the hearts and minds of citizens. It was not because politicians took wrong turns in the corridors of the *Landtag* or *Reichstag* that Germany stumbled into the political wilderness.

Orlow well describes the fate of Prussian government, but he defers an analysis of the players that determined that fate, from neoconservatives in Berlin to insurgent farmers in the countryside. As a result, this study is conceived largely in the passive tense. Prussian cabinets are the victim of circumstances beyond their control. Yet all this illustrates Orlow's point that Weimar Prussia was not Weimar Germany and accents his upbeat final note: that the distinctive collegiality of Prussian politics before 1933 provided a valuable fund of experience after 1945.

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DIETER GOSEWINKEL. *Adolf Arndt: Die Wiederbegründung des Rechtsstaats aus dem Geist der Sozialdemokratie (1945–1961)*. (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 25.) Bonn: J. H. W. Dietz. 1991. Pp. 660. DM 158.

In the light of German unification in 1990, the many "ifs" of history raise tantalizing questions. If the West German Social Democrats had won the first national election in 1949 and had not pursued Adenauer's policy of a close linkage to the West and rearmament, might the Soviets in the 1950s have backed an accord on German unification that only became a reality four decades later? Would the Social Democratic Party (SPD) have scored better in subsequent elections in a united Germany and remained in power, thus possibly producing a different European geopolitical design?

These ruminations arise as a result of reading the first published political biography of Adolf Arndt, a leading constitutional lawyer and SPD official whose views on the German state and unification helped to shape the SPD's policies in the formative years of the Federal Republic. Dieter Gosewinkel has the proper background to have written this authoritative volume, originally a dissertation for Freiburg University. A specialist in constitutional law and modern history, Gosewinkel is presently on the research staff of the Friedrich-Meinecke-Institut at the Free University of Berlin. He has thoroughly combed every archive that has published and unpublished materials on Arndt and has interviewed a number of people who knew him well. The result is a sympathetic, but also objective, portrayal of a man who rose swiftly into positions of power after World War II.

Arndt, born in 1904, came from a Prussian, conservative background. His father, a law professor, was Jewish, but converted to Protestantism. During the Weimar era, Arndt studied law at the University of Marburg, began an academic career, and became a judge in Berlin. During most of the Nazi era he was restricted to practicing as an attorney, having mostly Jewish clients. In 1944 he was forced to work as a tax auditor for a private company.

After the war he turned to the SPD, partly because of its stand against the Nazi regime. He became a leading official in the Ministry of Justice of the SPD-governed state of Hesse, drafted its constitution, and served as a member of the bizonal Economic Council and the Bundestag in 1949. Kurt Schumacher, SPD chairperson, relied on his skill and expertise, but also had great influence on Arndt's views on foreign policy. Arndt was more than the leading law expert of the SPD parliamentary group (*Fraktion*). His espousal of ethical socialism, protection of individual freedoms against a powerful state, the rights of the *Länder* against the central government, and the party's relation to the churches had an impact on the

party's theoretical positions as it abandoned the remnants of Marxism.

Arndt, a brilliant speaker, argued the SPD case on numerous occasions before the Constitutional Court, hoping that the court could put a brake on the conservative government. He scored some victories, but suffered defeats too. Arndt withdrew from politics in 1961 when his health deteriorated.

His death in 1974 ended the distinguished career of a brilliant intellectual who held high posts in the SPD but never aspired to top political office, helped shape West Germany's constitutional framework and legal system, and was one of the architects of the SPD switch from a confrontational to a constructive opposition course. Gosewinkel's definitive political biography is highly recommended.

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KEITH P. LURIA. *Territories of Grace: Cultural Change in the Seventeenth-Century Diocese of Grenoble*. (Studies on the History of Society and Culture, number 11.) Berkeley and Los Angeles: University of California Press. 1991. Pp. x, 267. \$40.00.

In this revised dissertation, Keith P. Luria makes a significant contribution to the literature of both popular culture and the French Counter Reformation. Agreeing with those who deny the validity of the terms "elite" and "popular" culture, Luria employs the idea of "village religion," specifically the interaction between church and village culture, as a vehicle to assess cultural change in the rural parishes of seventeenth-century Grenoble. His thesis is that the major cause of change was not outside intervention, but the internal dynamics of village culture itself.

Particularly sensitive to multiple meanings of symbols and practices, Luria stresses the vitality of village religion in the face of the Counter Reformation's demands for godly discipline. Whereas some have emphasized the imposition of episcopal absolutism on freewheeling and sometimes pseudo-Christian practices, Luria discovers the local adoption of practices usually associated with the Counter Reformation that antedate the movement. Far from succumbing to bishops' efforts to impose new rituals and behavior, villagers often resisted, ignored, or manipulated episcopal instructions.

Luria begins with a thorough geographical and religious description of the diocese. He then concentrates on the pastoral visitations between 1672 and 1707 of Étienne Le Camus, the first Counter Reformation bishop of Grenoble. The "encounter" of episcopal reformer with villagers recalls that of Europeans and Native Americans in the New World. Often appalled by what he found, Le Camus tirelessly strove to introduce Counter Reformation behavior and devotions. He might have been even more dismayed



had he been aware of how often villagers thwarted his wishes. Luria illuminates the various ways villages avoided the bishop's demands. He treats villagers sympathetically, seeing them as dynamic and active participants in creating their own culture rather than as passive recipients of clerical or political cultures.

Luria devotes two chapters to the cult of saints, the major ingredient in village religion. For the reformers, saints indicated the universality and order of Catholicism; for villagers, saints meant freedom from the hierarchy, a means to effect communal solidarity, but especially an avenue of supernatural aid. While some new Counter Reformation saints did gain prominence at the local level, Luria shows that villagers retained their traditional helping and healing saints and that the Counter Reformation cults of the rosary and of the blessed sacrament arrived before Le Camus. Thus, villagers themselves, not the hierarchy, chose certain Counter Reformation forms of religious expression.

To illustrate his basic contention that local reasons accounted for more religious changes than the Counter Reformation itself, Luria provides a fascinating case study of cultural change in one village, Entraigues, where two families dominated economic and religious life. Their rivalry, which spanned at least three generations, provided the major impetus for religious change. Villagers, not outside reformers, were responsible for the new devotions.

Luria has complete control of the archival sources and cogently mixes his research with theoretical approaches to religion and popular culture. Entraigues may not have been typical and Luria sometimes overstates his case for the ability of village culture to resist the church hierarchy, but his arguments are compelling and form a needed corrective to scholars who exaggerate the capabilities of the Counter Reformation hierarchy. His book is a model local study of cultural and religious change.

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GEORGE W. MCCLURE. *Sorrow and Consolation in Italian Humanism*. Princeton: Princeton University Press. 1991. Pp. x, 311. \$39.50.

George W. McClure has written an excellent survey of writings on sorrow and consolation in Italian Renaissance humanism. The author introduces his work with a summary of earlier examinations of psychological states of despair and consolation, including works about classical, patristic, and medieval consolation. McClure notes an important limitation in medieval consolation: Christians could only partially dispose of the sorrow of a believer, lest they deny the greater consolation of the afterlife.<sup>2</sup>

After a nod to the existence of model letters of consolation among the *dictatores*, the author discusses in detail the preeminent role of Francesco Petrarca by

devoting three chapters to the *trecento* humanist as self-consoler, public consoler in his letters, and universal consoler in his *De remediis utriusque fortune* (1366). Petrarca's validating of various psychological states is derived, according to McClure, from his poetic concerns. The humanist moved toward a more stoic position in the 1350s and 1360s, however, as he revived classical models. McClure argues convincingly for Petrarca's authority in succeeding generations and the central role of the *De remediis* as a store of philosophical arguments. Chapters follow on the more community-based consolation of Coluccio Salutati, fathers' self-consolations on the loss of their sons, two *quattrocento* clerical writers on consolation, Platonic consolation in Florence, and a summary conclusion.

McClure analyzes in detail well-known and obscure consolatory epistles, dialogues, books of psychological remedies, and handbooks of the *trecento* and *quattrocento*. He presents his views through discussions of texts, often with provocative themes, rather than through argued hypotheses. Among his themes are how specific emotional experiences of the humanists led to the legitimating of secular sorrow, the importance of the ideas of the dignity of man and the immortality of the soul in combating the asceticism expressed in the medieval concept of the misery of man, and how the *topoi* of dignity and immortality appeared first in consolatory genres and only later in fuller expression in other genres.

A final minor criticism: I would have preferred less repetitive summarizing and substantially more discussion of Francesco Filelfo's consolatory writings, which are acknowledged as some of the most significant of the Italian Renaissance, and more on McClure's views on the relationship of his findings to earlier scholarship on Renaissance sorrow and consolation, specifically that of Paul O. Kristeller and Charles Trinkaus.

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VOLKER REINHARDT. *Überleben in der frühneuzeitlichen Stadt: Annona und Getreideversorgung in Rom 1563–1797*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 72.) Tübingen: Max Niemeyer. 1991. Pp. xxii, 570. DM 166.

Providing food of acceptable quality in times of poor supply has always ultimately been a responsibility of the state, but this was particularly the case with Rome in the early modern period. A very large city without export industries in the middle of a territory largely bereft of a native peasantry, Rome was governed by the pope, an official committed by tradition to provide *abbondanza* to the poor. In concrete terms, this meant the ready sale of brown wheatbread of a good weight and for an acceptable price.

Although a few echoes of the *annona* of antiquity could be heard in the sixteenth century, the institu-

tion studied here is a creation of the later sixteenth century, at the culmination of the price revolution. The *annona* had its main storage facility in the Baths of Diocletian, where the wholesale price of grain as well as the price and weight of bread was regulated. In principle, grain was stored for emergencies, and in times of dramatic shortfall the *annona* commandeered harvests from the vicinity of the city and arranged for imports by sea from other parts of the Papal States. If all else failed, grain could be bought and freighted from Sicily and France, or from as far away as Holland, England, or even Danzig.

Volker Rheinhardt has exhaustively analyzed the records of the *annona*, and he clearly admires its quiet success for all but a few months of the more than two centuries it operated. Costs were sometimes disastrous to papal finances, but the preservation of cheap bread for the poor was one of the papacy's chief political obligations. Those popes who managed to keep brown bread cheap and heavy were celebrated, while those who cut weight or allowed prices to soar suffered *damnatio memoriae*. On the whole, Reinhardt estimates the *annona* managed to dampen retail prices by 5 to 10 percent over the period in question. Rome had an almost unique record of good supply, largely as a result of diligent effort.

The ultimate cost of the *annona* was not limited to papal deficits, however. In the Roman Agro, the region immediately around Rome, concentration of land into the hands of a few noble clans and institutions encouraged the progressive abandonment of plowland in favor of pasturage. This was partly due to a preference for cattleraising, paralleling landholding patterns in Latin America in the same era, but the existence of a command economy for wheat providing maximum prices but no minimum support price for heavy harvests encouraged the trend. On top of this, the paternalistic papacy subsidized a massive dependent poor class. In times of shortage, the rural population emigrated to Rome en masse, since bread could always be found there. As a result, the ability of Rome to feed itself from the Agro declined in the eighteenth century as a result of a planned economy.

From the middle of the eighteenth century, proposals flourished to reduce the role of the *annona* and accept the brutal operation of an open market for grain. Those who wished to preserve the institution proposed encouraging grain production in the Agro by homesteading neglected *latifundia* or even confiscating land from socially irresponsible owners. The conflict between landowners desiring to exercise their property rights and a fiscally exhausted state characterized eighteenth-century Rome just as deeply as *ancien régime* France, but only the destruction of the papal state by Napoleon would bring the *annona* to an end.

Reinhardt has produced a massive but readable book exploiting statistics to produce the best sort of economic history. In a time when the transition from a planned to a market economy is a living ordeal for

much of the world, a fresh look at the peculiar institutions of the early modern period is more than welcome. Reinhardt has resisted the temptation to make extensive comparisons, except briefly with other early modern towns in Italy and Germany. Still, any reader will be stimulated to draw parallels in other ages and cultures.

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DINO CINEL. *The National Integration of Italian Return Migration, 1870–1929*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1991. Pp. vi, 280. \$49.50.

Dino Cinel has written an interesting and insightful study of Italian emigration and return migration in the late nineteenth century. It will be useful for immigration historians, researchers, and policy makers as a guide to the basic literature on the subject, particularly the multivolume *Inchiesta parlamentare sulle condizioni dei contadini nelle province meridionale e nella Sicilia 1902–1910* (Faina Report), which is extensively used. Cinel's introductory chapters cover the history of the southern question and the national character of southern Italians; in later chapters he discusses the debate on emigration, return migration, and remittances in south Italy. He concludes with a study of the effect of government policy on the south.

In the nineteenth century, industrial countries exported manufactured products and services to less-developed countries and imported raw materials. Southern Italy exported labor and imported remittances—savings sent by overseas workers to relatives at home. The effect of migration and remittances for the Italian economy has been the subject of a long debate among Italian historians. Ruggiero Romano has argued that remittances had little effect on Italian economic growth when compared to the social and economic costs of migration. Ercole Sori, however, has maintained that remittances were important. At the macro level, they provided a surplus in Italy's international accounts, made possible larger imports of coal, iron, and cotton in a period of deficit trade balances during Italy's industrial revolution, and helped with the successful conversion of the consolidated debt in 1906. At the micro level, they led to improvements in the standard of living, helped peasants pay debts, support relatives, buy land, build houses, and so on. Similar effects have been observed in Croatia, Galicia, and Slovakia. Yet recent work suggests that Romano is probably closer to the truth.

Emigrant savings were sent back to Italy in several ways: via the Bank of Naples (after the reform of 1902), as international postal money orders, through private banks, by letter, and carried back by returnees. Only transmission through the Bank of Naples and by postal money order were statistically measur-

able, so that we have only estimates of the amounts involved. It is unfortunate therefore that Cinel has relied on the ISTAT national statistics institute study of 1952, which is seriously flawed for its estimates of emigrant remittances. The ISTAT study overstates the value of remittances by 50 percent between 1902 and 1913. The values then cited for the postwar years 1919–30 become astronomical because of violent devaluations of the lire at a time when remittances were actually declining. When billions of dollars are then confused with billions of lire (pp. 146–47), the exercise becomes a kind of elegiac dementia.

Although remittances played an important role in Italy's development, their potential was never realized. They contributed approximately 3 to 5 percent of Italy's national product and were about as important as tourism to Italy's balance of payments. The largest part of remittance income, however, was used for consumption rather than savings or investment. In fact, there were few opportunities for investment by peasants in the archaic, parasitical agricultural system of south Italy, and very little investment in small artisan enterprise that could have stimulated growth, as the government did not encourage it. The long-term effects of remittances therefore were slight.

Other aspects of Cinel's study raise interesting questions. Given the largely temporary character of Italian emigration to the United States, it is difficult to understand why repatriates are considered failures. One might argue that returnees were actually the most successful migrants.

Finally, we must accept Cinel's conclusion that emigration relieved population pressure but failed to solve the southern question. The significance of the remittance problem, however, is more complex. Southerners burdened with the heaviest agricultural debt in Italian history, accumulated between Joachim Murat and the kingdom of Naples in 1810 and the agricultural depression of the 1880s, were neither risk averse nor Schumpeterian entrepreneurs. Faced with a vicious cycle of underproduction, they migrated in order to recover their property. A catastrophic fall in agricultural incomes thus liberated the only abundant factor in the economy, labor.

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RETT R. LUDWIKOWSKI. *Continuity and Change in Poland: Conservatism in Polish Political Thought*. Washington, D.C.: Catholic University of America Press. 1991. Pp. xv, 313. \$39.95.

Rett R. Ludwowski's timely study focuses on the historical development of Polish conservative thought. He offers an astute analysis of the complex environment in which Polish conservatism arose, developed, thrived, responded to numerous challenges,

and persisted. As such, this book is a welcome complement to the numerous studies devoted to the development of Polish democratic thought, and it enhances one's understanding of the role of the new conservatism in contemporary Polish affairs.

The book consists of seven chapters and a brief conclusion. The first two chapters provide the historical and political backgrounds for the emergence of a "conscious conservatism" by the end of the eighteenth century, when the constitution of May 3, 1791, was drawn up. In chapter 2, the author carefully defines political terms and provides succinct introductions to leading Polish conservative thinkers and activists (including Archbishop P. J. P. Woronicz, Count Henryk Rzewuski, Prince Ksawery Drucki-Lubecki, the Kaliszans, and others). He comments on indigenous and foreign influences. He carefully delineates the emergence of a conservative creed by the early nineteenth century, while paying apt attention to the main rivals of the conservatives, such as Wojciech Gutkowski, Boleslaw Limanowski, and others.

In succeeding chapters Ludwowski argues that conscious conservatism became a dominant creed in the nineteenth century. He explores in some detail its operation in the absence of a sovereign state and its reactions to the dynamics of change throughout that century. The final two chapters focus on Polish conservatism and its proponents in the period between the two world wars and on its renaissance in postwar Poland. The author suggests that this renaissance had its roots in the crisis of communism and argues convincingly that the new conservative movement that emerged in the 1980s contributed significantly to the emergence of Solidarity and to Poland's peaceful revolution.

Ludwowski's study is a readable, informative, and informed melding of historical narrative and political analysis. His greatest strength is his effective presentation of political culture as a dynamic phenomenon, shaped both by a national historical consciousness and by the ongoing process of social, economic, and political activity. The earlier chapters (1–6) inform and also serve as a prelude to his most insightful chapter (7) and the conclusion, both of which focus on contemporary Poland.

Copious footnotes include references to Ludwowski's numerous earlier publications as well as to a wide variety of primary and secondary materials. The study is flawed by the omission of a bibliography, not necessary for the general reader perhaps, but certainly useful to the scholar. Editorial fine-tuning prior to publication could have eliminated recurring typographical errors (on, for example, pp. 5, 34, 171, and elsewhere) and other minor textual detractions (as on p. 88).

Overall, Ludwowski's study is a worthwhile introduction to Polish conservative thought for the general reader, the historian, and the political scientist. The subject has been generally neglected by scholars, who

focus more frequently on the development of Polish democratic thought.

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JAFF SCHATZ. *The Generation: The Rise and Fall of the Jewish Communists of Poland*. (Societies in East-Central Europe, number 5.) Berkeley and Los Angeles: University of California Press. 1991. Pp. x, 408. \$39.95.

In this meticulously researched, carefully constructed study, sociologist Jaff Schatz has delivered far more than promised in the book's title. For he has produced a concise yet comprehensive history of the communist movement in Poland from its inception to the decade of the 1970s while simultaneously fitting into its framework the story of a numerically small but qualitatively significant segment of Polish Jews who were arguably among the most committed "true believers" in the communist vision.

Schatz divides his study into four distinct parts that mirror the major segments of both this "generation's" evolution, and that of Polish communism itself. After first examining the general socioeconomic, political, and cultural profile of the Polish Jews and the reasons for, and process of, the conversion of some to the promise of communism, he then describes in detail the structure and functioning of the movement in Poland and the specific roles, challenges, and activities of its Jewish members prior to World War II. He notes that imprisonment played an especially pivotal role in reinforcing commitment and generating a sense of genuine solidarity among all communists, but particularly the Jewish comrades. The third part of the book, entitled "The Soviet Odyssey," focuses on the experiences of those Jewish communists who spent the war years in the Soviet Union, where conditions and government policies both reinforced and somewhat weakened their convictions and commitments to the cause. The final portion of the book chronicles their triumphant return to Poland as the new masters of the land that once scorned them, the turbulent and dangerous years of Stalinization, the 1956 "thaw" and its aftermath, and, finally, their rather sudden transformation from heroic pioneers of the new order into pariahs who threatened the very existence of "Socialist Poland" and who consequently found themselves driven from their homeland by the very system they had been so instrumental in creating.

Throughout his narrative, Schatz effectively blends detached objectivity with understanding for the positions of all parties involved in the various phases of his story. He skillfully combines exhaustive knowledge of previously published work with interviews of those few surviving Jewish communists to offer a complete profile of them and their cause. Schatz displays a keen sense of historical perspective that his clear, concise writing further enhances. Readers will

find his discussion of methodology in the appendix to be particularly helpful. Indeed, the only development that will prevent this book from becoming the definitive work on its subject will be the sudden appearance of substantial government or party documentation hitherto denied to outside investigators but available in post-communist conditions. Until that unlikely occurrence, students of Polish communism and the Jewish role in it need not look beyond this book.

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BRUCE F. PAULEY. *From Prejudice to Persecution: A History of Austrian Anti-Semitism*. Chapel Hill: University of North Carolina Press. 1992. Pp. xxix, 426.

Bruce F. Pauley has made an important contribution to our understanding of anti-Semitism in general and Austrian anti-Semitism in particular. His book, which focuses on the interwar years, provides the most comprehensive analysis of Austrian anti-Semitism to date.

Pauley explores the multiple causes and effects of the many varieties of anti-Semitism that manifested themselves in nineteenth and twentieth-century Central Europe. He tries to identify why different groups adopted specific forms of anti-Semitism and how each political party used anti-Jewish stereotypes and prejudices for its own purposes. He also attempts to juxtapose the realities of the Jewish situation in Austria, particularly in Vienna, with the distorted perceptions of anti-Semites concerning the Jewish role in Austrian society and the economy. After tracing the development of Austrian anti-Semitism through the *fin-de-siècle* era of Georg von Schönerer and Karl Lueger, Pauley evaluates the effects of World War I and the subsequent economic and political crises as contributing factors to a resurgence of anti-Semitism in the 1920s. He assesses the violent outbursts of anti-Semitism at Austrian universities, as well as anti-Semitic and anti-Zionist demonstrations and social discrimination against Jews, especially in tourist resorts.

Pauley portrays the political system of interwar Austria as permeated with anti-Semitism. Although he distinguishes between the milder use of anti-Semitic stereotypes by the Social Democratic Workers' Party on the Left and the much more extreme manifestations of religious and racial anti-Semitism by the Christian Social Party, the Austrian Nazi Party, and other minor political groups on the Right, the nuances often become blurred. He overstates the prevalence of anti-Semitism among the Marxists and thereby diminishes the venomous quality of Nazi anti-Semitism. Pauley places particular blame on the Catholic church and its Christian Social Party for legitimating and spreading anti-Semitism in Austria.

This book contains many villains and few heroes,



but it has a heroine, Irene Harand, whose motto, "I fight anti-Semitism because it maligns our Christendom," undoubtedly reflects the author's personal credo. Pauley argues that Austrians were consistently more extreme in their anti-Semitism than other Germans and that Germany frequently lagged behind Austria in implementation of anti-Semitic measures, even under the Nazis. He claims, not always convincingly, that Austria tended to be the first and foremost in Central Europe when it came to targeting Jews, although he does not fully explain why this was the case.

Pauley attempts to present a balanced account of both the perpetrators of anti-Semitism and their victims, but he sheds considerably more new light on anti-Semites than he does on Jews. He makes numerous minor factual errors in Jewish history, especially in comparative statements. He has some difficulty presenting statistical data effectively and sometimes relies on numbers of questionable validity. The book has an excellent index and a fine bibliography, but its abridged endnotes are not as helpful as they might be in identifying sources of information.

Pauley argues that instead of a democratic republic, Austrian Jews needed a strong, authoritarian government that would protect them, and he claims that Jews were better off with Engelbert Dollfuss as chancellor in the 1930s than under Ignaz Seipel in the 1920s. He also asserts that virtually all Jews wholeheartedly supported Dollfuss, and then Schuschnigg, whereas I would maintain that most Jews only supported these governments under duress and as the lesser evil to Nazism. Somewhat curiously, Pauley contends that "the most important reason for failure [to combat anti-Semitism effectively] was the relatively small size of the Jewish population in Austria as well as the small number of Christians willing to help them" (p. 258). I honestly doubt that having more Jews would have solved the problem of anti-Semitism in Austria any more than it did in Poland, but having fewer anti-Semites and more Irene Harands certainly might have helped.

This philo-Semitic history of Austrian anti-Semitism is not without its flaws, but it is well researched, provocative, and definitely worth reading.

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HELMUT GRUBER. *Red Vienna: Experiment in Working-Class Culture 1919–1934*. New York: Oxford University Press. 1991. Pp. x, 270. \$29.95.

With the dissolution of the Austro-Hungarian empire and the extension of universal suffrage to local elections, the city of Vienna came under the control of the Social Democratic Party (SDAP) and remained a "red" city until democratic governance went under amid cannon fire and civil war in 1934. The effort of the SDAP to reshape the city's social, cultural, politi-

cal, and economic life produced the successes and failures that are the concern of this book. Let it be said immediately that Helmut Gruber has produced a work of first-rate scholarly quality, one that seems to have not left a single stone (or pebble, for that matter) unturned; the notes and appendix take up nearly one-third of the volume. In its thoroughness it carries on where Charles Gulick's two-volume *Austria: From Habsburg to Hitler* (1948) left off. Gruber has also drawn on the voluminous scholarship about the interwar years produced by the postwar generation of Austrian social scientists.

Yet with all that, this is an exasperating work because it subjects the historical evidence so carefully and assiduously marshaled to an interpretation that not only rings untrue to someone who has lived through that period—as a child and teenager, to be sure—but also because it blindly bypasses most of what social scientists have been saying about social movements and leadership since at least the end of World War II, if not earlier. It seems to me that the historian interpreting a particular set of historical experiences has an obligation to place that interpretation within an existing universe of social theory. Gruber offers such an interpretation. But in the absence of any effort to legitimize that interpretation in the context of existing social theory, one is entitled to say: "Well, that is *your* view." Let me explain.

At the heart of the book are four chapters in which Gruber examines, successively, municipal socialism (housing, health, and education); socialist-party culture (the power of ideas, music, theater, and sports); leisure time (the conflict between mass and commercial culture); and the family (the role of women and issues of sexuality). In these chapters Gruber offers a truly encyclopedic picture of the SDAP's effort to create a socialist life, but all set in the context of a sort of *trahison des clercs* framework: the working-class movement was betrayed and misled by a group of bourgeois intellectuals who despised working-class culture as it existed and who attempted to force their bourgeois, university-educated ideas on a working class that shared none of them. In the end Gruber really denies the very possibility of leadership in human organizations, for to him all leaders do is try to impose their values on their followers, and in this way they falsify the nature of the enterprise in which both leaders and followers are engaged.

The *trahison* quality of Gruber's interpretation is particularly evident in the section on Austromarxism, a theoretical construct alien to the mass of workers, who shared none of the characteristics of the secularized Jewish middle-class intellectuals who concocted it. Gruber's hostility is also reflected in the chapter on the family, subtitled "Invasions of the Private Sphere." He charges the party leadership with desiring to foist their middle-class, scientific notions on the mass of workers and ascribing near-totalitarian qualities to leadership efforts. Thus, what mars this study is a failure to entertain seriously any systematic con-



sideration of the nature and structure of leadership in modern societies. If Gruber had considered such issues, his book would have escaped much of the strictures leveled against it.

Gruber also engages in quite a bit of hindsight management, more than is really justified by the evidence he musters. It is all very well to say that the SDAP should not have dampened the revolutionary drive of the workers, that it should have pushed for workplace democracy, and that it should have insisted on full separation of church and state rather than be satisfied with a partial separation. Would it have been preferable for Austria to follow Munich and Budapest and for Otto Bauer to have suffered the fate of Kurt Eisner and Bela Kun? Had workplace democracy been realized anywhere else in the industrialized world until well after World War II? Even if the SDAP had been able to push separation of church and state over all constitutional and legislative hurdles, would it have been able to make it work in any meaningful way given Austria's political constellation in the interwar years?

From being condescending regarding the SDAP's political ability to overcome its political limits, Gruber becomes quite censorious regarding the issue of "cultural limits." Why does he find it appropriate to deny the role of elites in any culture, including that of Austria? By what notion of social organization are all elites "paternalistic" almost by definition? And why does Gruber romanticize mass culture (by which he means worker subculture) while denying that he is doing so? He rejects the possibility of countercultural enclaves being formed and continuing over time; on what grounds? Given his norms, is EuroDisney to be preferred to the SDAP and its efforts to make available to a wider public the works of Upton Sinclair in translation in lieu of the romantic tripe of a Hedwig Courts-Mahler?

In the concluding chapter of the book there is a pervasive tendency to consign all social experimentation to the trash heap of history. Such rewriting of history should not be linked to the sort of scholarly excellence that Gruber has demonstrated in this work, as well as in his long list of previous professional endeavors.

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JOHN HIDEN and PATRICK SALMON. *The Baltic Nations and Europe: Estonia, Latvia and Lithuania in the Twentieth Century*. New York: Longman. 1991. Pp. x, 224.

The work by John Hiden and Patrick Salmon provides a welcome survey of the twentieth-century history of Estonia, Latvia, and Lithuania, which until recent times remained a virtually forgotten corner of Europe. The dispassionate approach and not insignificant body of factual material provides a good

general introduction to the subject. The authors' admitted unfamiliarity with the indigenous languages, however, circumscribes their effort considerably and doubtlessly contributes to the rather striking imbalance in organization and scope.

A consistent effort is made to note the general differences between Estonia and Latvia on the one hand and Lithuania on the other. While these differences have decreased during the last half of the twentieth century, they nevertheless remain significant. Yet a disproportion of attention on Estonia and Latvia to the detriment of Lithuania clearly permeates the entire presentation.

A major imbalance appears in the structure of the work. Slightly more than half the volume is devoted to the prewar period; one-third of this section deals with the national renaissances of the three Baltic peoples dating from the second half of the nineteenth century. The extremely significant years of World War II are covered in a rather short chapter of sixteen pages. The subsequent four decades of Soviet occupation receive a mere seventeen pages while the last forty-five pages are devoted to the period of the national fronts, 1988–90, which heralded the reemergence of independence.

The prewar section is quite well researched and appears to be based heavily on an earlier monograph by one of the coauthors on the interrelationship of German political and economic policies in the Baltic. At times, especially insofar as Lithuania is concerned, the treatment is fraught with error. The statement, for instance, that the authoritarian president of Lithuania reorganized his Nationalist Party along Nazi lines after 1933 belies an unfamiliarity with the subject (p. 54). Its organization predates 1933, and were a resemblance to any foreign model to be made, Kurt von Schuschnigg's Austrian nationalists provide a far closer analogy. In comparison with the first half of the work, the chapter on the war years is cursory. Developments during the first year of Soviet occupation, 1940–41, are virtually absent.

The paucity of attention to the postwar Soviet period is the striking shortcoming of the work. Some significant topics are hardly touched. The postwar guerrilla resistance, for instance, is noted in a single paragraph based, for the most part, on a recent popular account of M16 activity in the region that dealt primarily with Latvia, where resistance proved by far the least developed. There is bare mention of the movement in Lithuania, which lasted into the early 1950s and involved up to 50,000 partisans. The events of those years continue to exert a significant role on the Lithuanian national consciousness and even continue to color its contemporary political activity.

Some of the data in the postwar chapter is dated. Lithuania is accurately characterized as having remained less urbanized than its two neighbors (p. 131). The figure of urbanization provided, 50.2 percent in 1970, however, differs significantly from that of 68.8

percent in 1990. The gap among the three countries in this respect has narrowed significantly over the two preceding decades. Most of the description of Lithuania's economy is based on a publication from 1965.

The survey of the reemergence of the Baltic states from the crumbling Soviet state is well done. The chapter on the economic considerations in the process of Baltic secession is particularly good. Unfortunately, the volume appeared prior to the coup in Moscow in August 1991 and could not encompass the natural denouement of the Baltic renaissance, the achievement of political independence.

In addition to the imbalance of attention devoted to certain periods, there is also a marked shift in approach that surfaces in the middle of the volume. The expressed aim of the authors is to view Baltic developments from a European perspective. The interwar period is indeed presented in such a manner. The vantage, however, shifts noticeably in the chapter on the war years and changes entirely in the remaining portion of the volume. It almost appears as if two distinct works have been joined by a transition chapter on the war years.

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PETER K. CHRISTOFF. *An Introduction to Nineteenth-Century Russian Slavophilism*. Volume 4, *Iu. F. Samarin*. Boulder, Colo.: Westview. 1991. Pp. vii, 469. \$54.95.

The present study is the fourth and final volume in a series that includes published studies on A. S. Khomiakov (1961), I. V. Kireevskii (1972), and K. S. Aksakov (1981). Peter K. Christoff's interest in Iu. F. Samarin (1819–76) is more with the Slavophile movement than with the biography of one of its key representatives, more with those contributions by Samarin that lent characterization to Slavophilism than with a comprehensive review of his writings, and still more with Samarin's formal ideas than with the values and ethos that informed and shaped those ideas. This fourth volume, generally, details the "descent" of the Slavophiles from the intellectual and moral convictions of the 1840s to the practical principles that impelled some Slavophiles to assume roles "among the prime movers in bringing about the emancipation of the serfs in 1861" (p. 3).

The contents distinguish between "early" or "classical" Slavophilism of pre-1861 vintage and "reformist" or "progressive" Slavophilism of the 1860s. Like so many other Slavophiles, Samarin was not raised in some boorish gentry nest, but was well schooled at home in the fundamentals of European culture. His years at Moscow University (1834–44) coincided with the period of the pervasive influence of Hegelianism, arguably the most important European philosophy to have currency in nineteenth-century Russia. Samarin's subsequent bureaucratic career in the Baltic provinces, particularly Riga, and Kiev (1844–53) was

primarily concerned with the serf question. As the twinned issues of civil emancipation and disposition of land became paramount in the mid and late-1850s, Samarin was uniquely qualified to adapt Slavophile philosophical ideas about the Russian peasant and commune to the realities of reform (p. 189).

Slavophile reformism of the 1860s is described by reference to the influence of a "troika" of Samarin, V. A. Cherkassky, and A. I. Koshelev. Christoff is cogent and persuasive in his analyses of Samarin's numerous memoranda on specific problems with the emancipation. As Samarin served as a government member of both the provincial committee from his native Samara and then the general Editorial Commission, his general involvement in the great reform of February 19, 1861, is indisputable and is a story well told by Christoff. At this chronological point, the author concludes with chapters on the general relationship between Slavophilism and the "native soil" movement (*pochvennichestvo*) of the 1860s and between the Slavophilism and Pan-Slavism of the last third of the century.

The issue of Slavophile reformism is less a question of the representatives' influence on emancipation than the reverse: the changed circumstances that required substantial revision of the Slavophiles' original philosophy. The "troika" is a curiosity. Aside from Cherkassky's dubious credentials, the differences between Samarin and Koshelev seem evidence of the decay and disintegration of classical Slavophilism, not its continuation. The argument for a "progressive" phase in post-1861 Slavophilism leads to a number of troublesome claims for the association between Slavophiles and the varied political philosophies of the 1860s. The comparisons only dilute the distinctiveness of the references and the very credibility of the comparisons. Also, if we assume Christoff's argument that pre and post-1861 Slavophilism are two parts of the same whole, his argument for a progressive, later Slavophilism is a license to scour the texts of the "early" Slavophiles for evidence of societal commitments and abolitionist sentiment. Yet the substantial scholarly literature on classical Slavophilism bears witness to the primarily conservative content of a movement whose social interests were secondary and derivative of philosophical principle rather than social actualities. Curiously, the magisterial interpretation of Andrzej Walicki rates mention in only two footnotes. The net result is a problem of focus and integration. Christoff's broad argument about the nature and attributes of the movement so dims the distinctions between Slavophilism and other contemporary movements as to diminish the Slavophiles' particular contribution to the evolution of Russian conservative thought.

The intellectual origin of Slavophile thought is a major problem for every scholar. Christoff's main interpretive principle is that Samarin and many other Slavophiles "struggled" with Hegelianism in their university years and then "were won over from He-

gelianism to religion and Slavophile Orthodoxy" (p. 234). While many Russianists interpret figures in Russian intellectual history without reference to their debts to Western and Central European forebears, the interpretive consequences are inevitable. The Slavophiles' "rather baffling romantic-utopian adulation of the *narod* [people]" (p. 432) and "idealization of the Russian peasant order" (p. 195) refer to staples of Slavophile philosophy but are necessarily unexplainable without reference to the influence, in whatever transmuted form, of German Romanticism. More generally, the Slavophiles' advocacy of a communal order for the peasantry and church and their endorsement of personal liberties are deemed to be "incompatible" (p. 128) only by the absence of an appreciation of the German idea of freedom, an idea whose altruistic bases allow a ready reconciliation of the otherwise irreconcilable. Slavophilism is not only a particular chapter in Russian intellectual history but also a part of and a substantial contribution to European intellectual history.

Regardless of interpretive differences over "broad" and "narrow" opinions of Slavophilism, the overall import of this book is clear. Christoff, one of our most learned Slavists, is to be congratulated for the successful completion of his thirty years' labor of love and intellectual challenge with Russian Slavophilism.

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B. V. ANAN'ICH. *Bankirskie doma v Rossii 1860–1914 gg.: Ocherki istorii chastnogo predprinimatel'stva* [Banking Houses in Russia, 1860–1914: An Outline of the History of Private Enterprise]. Leningrad: Nauka. 1991. Pp. 196. 4 r. 10 k.

In this fascinating book on banking firms in prerevolutionary Russia, B. V. Anan'ich uses blatantly non-Leninist terminology—"private enterprise" in his subtitle—and, unlike Soviet scholarship before the era of perestroika, pays grudging respect to those who sought to bring imperial Russia into the world economy on terms that would protect the country's natural resources from unfair exploitation by Europeans and Americans. There are no references to Lenin, whose works Soviet historians previously cited at every opportunity despite his lack of expertise in Russian economic history. Instead, the notes contain many useful references to the fine study of Russian banking by Isaak I. Levin. The change from Lenin to Levin is small—only one letter—but it shocks the reader every time.

Anan'ich raises a simple question. In an age when private banking firms in Europe and America attained world-wide influence, did Russia produce its own "Rothschilds, Mendelssohns, Morgans, or Bleichröders"? To answer this question, he reviews the rise and fall of four important banking families. The

founder of the house of Stieglitz served as the "court banker" of the emperor from the era of Napoleon to mid-century, and his son headed the State Bank, center of the reformed financial system, from 1860 to 1866. The Gintsburg family, tied by marriage to various bankers in Europe, consistently pleaded the cause of Jewish emancipation and assimilation in Russia. In contrast, members of another Jewish banking family, the Poliakovs, struck political alliances with the most reactionary tsarist politicians in a variety of schemes to strengthen the Russian merchant marine and gain financial control of railroads in the Balkans. Finally, one of the most outspoken liberals of the late imperial period, Pavel P. Riabushinskii, headed a family enterprise that achieved prominence not only in textile production and banking but also in the rationalization of the Russian flax and timber trades.

Anan'ich's biographical focus, almost unknown in Soviet scholarship before 1985, brings Russian capitalists to life. His use of previously neglected archives in Moscow and New York also does a distinct service to scholarship on this obscure subject. The drawback of this focus on four prominent firms and a variety of smaller ones is that the author cannot do justice to many important questions of individual motivation. Admirable though these *ocherki* (essays, outlines, sketches) may be, they only whet the reader's interest in a series of comprehensive case studies of major banking firms in the Russian empire. Such a project will take many years to complete after decades of neglect at the hands of Soviet historians.

As for the main theme of his work, Anan'ich concludes optimistically that Russian counterparts of the great European and American bankers did indeed flourish under the tsars. To his credit, he admits the unique circumstances in which Russian bankers operated, primarily the legislative and regulatory fetters of the tsarist government and the weakness of the corporate banks, which depended on unincorporated banking firms to coordinate the creation of multisectoral enterprises in banking, railroads, textiles, or commerce, as in the case of the Poliakovs and the Riabushinskiis. He does not, however, stress other institutional contrasts between Russia and the West. The chronic financial weakness of the multimillion-ruble railroads that were designed—unsuccessfully, as the world learned between 1914 and 1921—to hold together the largest country in the world contrasted unfavorably with the condition of railroads in Europe and America in the late nineteenth century. Only in 1916, at the very end of the capitalist period, did representatives of banking firms meet to discuss their common interests; and their congress owed its existence not to a banker but to a Moscow professor who disapproved of financial abuses perpetrated by joint-stock banks, particularly those in St. Petersburg.

The classic dilemma of Russian capitalism—financial dependence on the same autocratic state that hindered rational economic development—does not

come into focus here as clearly as it does in the works of Alfred J. Rieber, Ruth A. Roosa, and James L. West on Russian merchants, and in Steven G. Marks's study of the Trans-Siberian Railroad. The institutional immaturity of Russian capitalism hovers in the background of this account instead of occupying center stage.

Paradoxically, the main contribution of Russian business history, now in its infancy, may well be to demonstrate the weakness of capitalism under the tsarist regime. This empirical finding would have theoretical implications for the comparative sociology of revolutions exemplified by the work of Barrington Moore, Jr., Theda Skocpol, and Tim McDaniel. If the social upheaval of 1917 did not represent a revolt against capitalism because capitalism as such had not yet grown to maturity, then what is the significance of the Russian Revolution?

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RONALD I. KOWALSKI. *The Bolshevik Party in Conflict: The Left Communist Opposition of 1918*. (Series in Russian and East European Studies, number 14.) Pittsburgh: University of Pittsburgh Press. 1991. Pp. x, 244. \$34.95.

Ronald I. Kowalski provides a thorough coverage of one branch of the Bolshevik Left that responded with dismay or outrage at Lenin's attempts to hammer out a "real existing socialism." Kowalski traverses fairly familiar terrain in describing the Left Communist position on the Brest-Litovsk Treaty, the organization of industry, agrarian policy, and the structure of the revolutionary state. Although some Left Communist positions are better known than others, Kowalski's contribution is to put all the pieces together into a useful, concise (188 pages of text) handbook on the movement. He also tries to round out the picture by moving outside of the top echelon of the party and its seemingly endless debates. Kowalski presents a careful reconstruction of the Left Communist position at the local and regional levels—the Moscow region, the Urals, and the Ukraine. His sources include often obscure and difficult to obtain local newspapers, like *Izvestiia Ural'skogo Oblastnogo Soveta*.

Kowalski adopts an even-handed approach in examining the concepts and proposals of the Left Communists; his conclusions, however, are unequivocal. Kowalski suggests that the Left Communist strength lay in "their forebodings that the recrudescence of authoritarian, bureaucratic practices in the administration of industry and the state encouraged by Lenin's policies could only lead to the degeneration of the revolution" (p. 184). Yet when the Left Communists' own position is bared to scrutiny, Kowalski argues, it in turn reveals fatal flaws and cannot readily be accepted as a democratic-socialist alternative to Lenin. The Left Communists' insistence on

central planning was incompatible with the devolution of power to the workers at the workshop level; their unremitting hostility to the peasants contained at least an implicit acknowledgment that force would have to be used to carry out their agricultural schemes; their solution to bureaucratism was to purge the party of non-proletarian elements. "The ideological preconceptions of the Left Communists," Kowalski concludes, "would have spawned a centralized, bureaucratic system, not an emancipated society in which power was diffused to the workers" (p. 188).

Kowalski is convincing in his argument that there were theoretical shortcomings in the Left Communist positions. Nevertheless, a number of questions arise. First, is there any evidence of what the Left Communists were like in practice? The sections on the regions—Moscow, Urals, Ukraine—are disappointing in this regard because the focus is still largely on debates rather than on "hands-on" policy. Particularly frustrating is the section on the Urals, because Kowalski stops his account precisely at the point where the reader is led to believe that conditions were ripe for worker management. Why then did the experiment ultimately fail?

Second, did not the purging of the Left Communists itself help precipitate the degeneration of the revolution and the rise of authoritarianism? What is Lenin's responsibility in all of this? In Kowalski's story, Lenin comes out time and again as accepting "harsh reality"; it is possible that Lenin's crude dismissal of the Left Communists—their fears as well as hopes—made the reality even harsher.

Third, what happens to utopianism or radicalism once thwarted? The shift to NEP (rather surprisingly, there is no discussion of War Communism at all) created an undertow of radical discontent that burst forth with a vengeance under Stalin. The purging of the Left Communists did not necessarily purge the Bolshevik Party of their ideas. Did the Left Communists pave the way for Stalin's "revolution from above" or would they have recoiled in horror?

Perhaps Kowalski is too careful a scholar to be tempted by such speculative—albeit intriguing—questions. It is a mark of his achievement that readers should want to engage in a further dialogue with him.

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LARRY E. HOLMES. *The Kremlin and the Schoolhouse: Reforming Education in Soviet Russia, 1917–1931*. (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 1991. Pp. xv, 214. \$25.00.

The content of Larry E. Holmes's short book can be briefly summarized. The new leaders of Narkompros (Commissariat of Enlightenment) aimed to introduce idealistic, experimental, and utopian educational policies. These policies would have—in their opinion—



contributed to the creation of a new socialist humanity. They failed. They had to battle against those who demanded a narrow, vocational education suitable for workers, and, above all, they had to work against a backward, impoverished society. Teachers and parents had little patience for abstract schemes such as the "complex" and "project" methods; they wanted the children to acquire the rudiments of knowledge, to learn to read and write and count. During the waning days of NEP in 1926 and 1927, the leaders of Narkompros were ready to compromise, but during the "cultural revolution" a new utopian wave buried the moderates.

None of these propositions is particularly surprising or new. Holmes, however, is a reliable and patient scholar who spent years in archives and is well acquainted with the secondary literature. He is therefore able to introduce bits of interesting information concerning the extraordinary poverty of the teachers, the backwardness of the countryside, and the weakness of party control. His judgments deserve attention.

But perhaps Holmes likes the archives a bit too much. The reader wishes that at times the author would get away from his documents and contemplate the significance of the issues raised and the story told. None of the major figures in this story come to life. We get no understanding of who these Bolsheviks were, how their educational ideas fit into their general world view, and how the battle over educational issues related to the life-and-death struggle for power taking place at the same time. It would also help if the author placed the accomplishments and problems of the educational establishment of the 1920s within the context of the history of Russian education. After all, the problems did not start in 1917, and the foundations of Soviet education created during the NEP period did not altogether disappear in the age of Stalin. Putting the topic in a larger context would also make his book more readable; the reader needs to know what is of interest in these seemingly obscure issues.

In his brief conclusion, to be sure, Holmes does attempt to make a contribution to the discussion of larger historiographical questions. Here, however, he fails. He claims that his study "corroborates the scholarship of Roberta Manning, J. Arch Getty, and Lynne Viola, who pointed to limits of the regime's power over society" (p. 144). There is hardly any need for corroboration, for no historian, as far as I know, ever claimed that the Bolsheviks had successfully introduced their utopian schemes. The author's opinion that there was a measure of popular support for radical change (p. 145) may be correct, but it comes to me as a complete surprise, for the book has demonstrated nothing of the kind.

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STEPHAN MERL. *Bauern unter Stalin: Die Formierung des sowjetischen Kolchossystems 1930–1941*. (Osteuropastudien der Hochschulen des Landes Hessen, first series, Giessener Abhandlungen zur Agrar- und Wirtschaftsforschung des europäischen Ostens, number 175.) Berlin: Duncker und Humblot. 1990. Pp. 512.

This volume is a sequel to Stephan Merl's previous publications, *Die Agrarmarkt und die Neue Ökonomische Politik* (1981) and *Die Anfänge der Kollektivierung in der Sowjetunion* (1985). In this hefty book, as in his earlier ones and their companion volume, *Sozialer Aufstieg im sowjetischen Kolchossystem der 30er Jahre?* (1990), Merl has painstakingly analyzed massive data and produced an impressive study that will remain for some time a standard work on Soviet agriculture in the 1930s.

Merl seeks to answer rather traditional questions, such as whether the collective farm system served state interests without destroying collective farmers, and whether the collective farms were viable as production units. The answers are not surprising: the collective farm system was forced to serve state interests by means of permanent terror; and the collective farms were not viable agricultural forms. Throughout the book Merl examines many other historiographical issues of the Soviet 1930s, but overall his strongly negative conclusions on the collective farm system tend to confirm what has been discussed in the West. The strengths of Merl's book, then, lie in the gathering of massive data and a careful analysis of them.

Merl argues his case by following, by and large chronologically, the development of the collective farm system. The first stage, 1928–32, was a period of terror, of the "elimination of the kulak as a class," in which rich peasants were dispossessed and others were forcibly driven into new collective farms. This was done with such systematic terror and without any regard for the lives of peasants that the whole system threatened to collapse in famine. Only then did the party leadership make concessions and compromises. Still, the famine took millions of lives.

In the second stage, a period of consolidation of collective farms (1933–38), the collective farmers were allowed to live on two forms of meager income. To prevent them from dying the government permitted collective farmers to divide 10 to 15 percent (20 percent in exceptional cases) of threshed grain among themselves (pp. 357, 375, 465). Contrary to the traditional understanding of the labor-day system, the residual principle of payment was actually not applied here. This distribution of grain, implemented regardless of state plan fulfillment by the collective farm, was meant to encourage prompt threshing; it came to assume the form of minimum wages. Simultaneously, the state had no alternative but to encourage the private plots and livestock of collective farmers. These forms of nonsocialized production provided in most cases the bulk of both cash



income and foodstuffs (except for grain) for the farmers.

The private productive activities of farmers, however, were not conducive to the development of collective farming. These activities were encouraged only insofar as the selling of animal products and potatoes was necessary for the farmers to survive. Once the collective farms were consolidated, the same impulse for state control that led to the collectivization of grain and other important technical crop production drove the party and the government to attack all private activities. (Merl provides here an insightful account of a brutal liquidation of *khutory* that took place at this time.) In this third stage (1939–41) of *kolkhoz* development, with the threat of war evident, the collective farmers were pushed back to a bare subsistence level. The destructive attack on the private plots was halted only by the German invasion and the onset of war.

The overall performance of agriculture thus proved singularly bad. In Merl's view, it was the system of collective farming that discouraged hard work and promoted the lack of labor discipline. Collective farms could not have become viable production units. Production remained stagnant while the state-forced marketing of grain and other agricultural produce increased substantially.

In his conclusion, Merl touches on the implications of the Soviet experience for postwar Eastern Europe. Hungary, Poland, and former East Germany learned a lesson from the Soviets and constructed their agricultural policies accordingly.

Merl has made little use of Soviet archival material (save the Smolensk archive). New archival data are likely to modify some of Merl's arguments, but his overall conclusions will remain valid.

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#### NEAR EAST

LEILA AHMED. *Women and Gender in Islam: Historical Roots of a Modern Debate*. New Haven: Yale University Press. 1992. Pp. viii, 296. \$30.00.

Leila Ahmed examines discourses about women and gender at three crucial moments of Islamic Middle Eastern history: Arabia at the time of the Prophet, Iraq during the Abbasids, and modern (nineteenth and twentieth-century) Egypt. The book is divided into three parts. Part 1 deals with the pre-Islamic Middle East. Ahmed asserts that women's social status began to decline with the rise of urban society and the archaic state (3500 B.C.). Islam, she argues, inherited the misogynist and oppressive models for treating women that were prevalent in Mesopotamia and the Mediterranean Middle East; it "did not bring radical change but a continuity of the life-styles already in place" (p. 33).

Part 2, entitled "Founding Discourses," analyzes in chronological order gender ideologies during the rise of Islam (620–650), the transitional age (650–750), the Abbasid period (750–1258), and the period of Medieval Islam. Largely on the basis of Islam's sacred texts (Qur'an and Traditions) and near contemporary biographical data, Ahmed contends that in Islam's early years there was a wide range of women's rights and social roles that society considered "proper." Moreover, she argues, the sacred texts themselves, which derive from this period, contain an ethical vision that is "stubbornly egalitarian" (p. 63) and that existed in tension with their pragmatic rulings, which sanctioned highly hierarchical social institutions, in particular marriage. It was in the Abbasid period, when texts that bore the stamp of the assumptions and practices of the highly misogynist male elite came to be regarded as the core prescriptive texts of Islam, that this ethical egalitarian voice was effectively silenced. Ahmed briefly refers to social movements that espoused more egalitarian interpretations of the gender roles (such as the Kharijis and Qarmatians) to prove her point that even contemporaries developed less-sexist interpretations of the sacred texts.

Why did Abbasid society emphasize the androcentric teachings about women in Islam rather than Islam's egalitarian voice? In analyzing causation, Ahmed seems to give too much weight to otherwise unexplored processes of cultural fusion and assimilation due to invasion, inheritance, and increased interaction between different peoples. The cause she highlights most, however, is one unique feature of Abbasid social organization, "the availability and easy acquisition of women and slaves and objects" (p. 86). Thus, Ahmed denies universal validity not to Islam's sacred texts themselves but to Abbasid interpretation and codification of the gender meanings of those texts. She concludes, maybe somewhat naively from the perspective of historical method, that "Had the ethical voice of Islam been heard . . . it would have significantly tempered the extreme androcentric bias of the law, and we might today have a far more humane and egalitarian law regarding women" (p. 88). This gender ideology, however, which sanctioned a social system that "combined the worst features of a Mediterranean and Middle Eastern misogyny with an Islam interpreted in the most negative way possible for women" (p. 128), was not seriously challenged until the nineteenth century. "Middle Eastern women," Ahmed categorically states, "have no cause to regret its passing."

In part 3, Ahmed takes the reader to modern Egypt. Under the title of "New Discourses," this section analyzes the clash between the old discourse and new ones, forged in the context of Western colonial domination and indigenous, nationalist, middle-class debates about modernization and westernization. Ahmed analyzes the new discourses on women against the background of colonial capitalist

change and of new state policies toward education, health, and labor. How did the issue of women's proper social roles become inextricably linked to issues of national advancement and cultural progress and authenticity? Ahmed traces the linkage between women, Islam, and "progress" to what she calls the colonial narrative, "in which the veil and treatment of women epitomize Islamic inferiority" (p. 163). This colonial narrative in turn called forth its opposite, a Muslim, Arab narrative of resistance, in which the veil came to symbolize the dignity and validity of all indigenous customs, in particular those relating to women. Ahmed discusses in detail first Egypt's earliest feminists, and then contemporary Islamist women, who at least partially, Ahmed contends, pursue the feminist objectives of "female autonomy and subjectivity." In comparing the objectives of Egypt's unveiled and newly veiled women, Ahmed emphasizes similarities. Both groups put their faith in Islam's ethical, egalitarian voice, and both are, at serious peril to their own future, ignorant of and oblivious to the authoritarian and sexist political objectives of most organized Islamist groups and movements.

In her conclusion Ahmed calls for a feminism that is "vigilantly self-critical and aware of its historical and political situatedness," and thus avoids stereotyping the Muslim "other." She rejects a discourse that "traps the issue of women with the struggle over culture" and measures ideas and practices by their tribe of origin rather than their merit. And she reaffirms her belief that the sexist understanding of gender articulated in the written corpus of establishment Islam can be challenged in the name of Islam's ethical voice.

While Ahmed's book is an inspiring tour de force, it is uneven and even fragmentary in the geographical and chronological coverage of its chosen subject matter. Its focus, moreover, is not always sharp, as it veers between an analysis of changing gender ideologies and Muslim women's changing social roles, without consistently showing the links between these two. Also problematic, at least for the premodern period, are the historical explanatory models Ahmed applies; these, as referred to above, are often idealist and simplistic. Although it is true that a work of synthesis cannot be stronger than the literature on which it is based—and this explains most of the gaps and flaws of this study—one would still expect a more cohesive historical method and vision. Nevertheless, this courageous and provocative study is a formidable historiographical milestone, on whose strengths we must learn to build and on whose weaknesses we must work hard to remedy.

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NIKKI R. KEDDIE and BETH BARON, editors. *Women in Middle Eastern History: Shifting Boundaries in Sex and*

*Gender*. New Haven: Yale University Press. 1991. Pp. xii, 343. \$35.00.

This volume surveys the multiple and dynamic roles of women and gender in Middle East history from the rise of Islam to the present. Far from a static set of stereotypes, the history of women in the Middle East has proven dynamic and complex. As Beth Baron notes in her opening comments, the study of women in the Middle East reveals a dynamic process of change characterized by both expansion and contraction of gender boundaries. Historians and social scientists, drawing on textual sources and field work, historical documents and literature, Middle Eastern and Western scholarship, demonstrate that it is as deceptive to posit a monolithic, essentialist approach to the status of women in Middle East history as it is to do so in understanding Islam. Nikki Keddie's introduction, "Deciphering Middle Eastern Women's History," provides a splendid overview of many of the institutions, themes, issues, and sociopolitical changes that are central to understanding the changing roles of Muslim women and that form a backdrop to chapters that range from the early Islamic and Mamluk periods to modern Turkey, Iran, and the modern Arab world.

Scholarship on Middle Eastern women remains methodologically diverse and divided: ahistorical and historical, ideological and socioeconomic. Deniz Kandiyoti underscores the issues and offers an alternative approach. Kandiyoti questions both the privileging of Islam and the adequacy of the term patriarchy, preferring "different systems of male dominance." She effectively employs a comparative analysis to demonstrate why and how Islam accommodates itself differently to patriarchy in diverse cultural contexts.

The diversity of Muslim interpretations and life in early Islam are informed by text and contexts. Leila Ahmed discusses the differences between the Qur'an and the development of Islamic legal texts as well as differences in Muslim practice by contrasting women's status in legal texts and their less restricted roles in Sufism and "heterodox" Islam. Denise Spellberg's study of the diverse readings of Aisha's role in early Islam and Paula Sanders's analysis of the accommodation of Islamic law and society to hermaphrodites demonstrate the diversity and flexibility of the Islamic tradition to gender roles.

Similarly, although at one level one can assert that Mamluk society was male-dominated, in reality women often played an active role in public life both because they willed it and because of historical circumstances. The differences between normative shariah ideals and social realities are illustrated in several studies by Huda Lutfi, Carl F. Petry, and Jonathan P. Berkey. Lutfi shows how the life of women in Cairo, as revealed in a fourteenth-century manuscript, contrasts with the prescriptions of Islamic norms. Petry demonstrates that women enjoyed considerable social

influence and power due to their roles as custodians and managers of family property. In a nuanced study, Berkey discusses how, despite restrictions, numbers of women came to be found among the prominent teachers and scholars in early Islam.

Modern Middle East history has been dominated by successive political and ideological forces: colonialism, liberal nationalism, Arab nationalism/socialism, and contemporary Islamic revivalism. Emerging nation-states witnessed governments that attempted to improve the status of women through legal and educational reforms. Nermin Abadan-Unat analyzes the impact of Attaturk's thoroughgoing secular legal and educational reforms, a mixed track record affected by class differences as well as the challenge of contemporary Islamic revivalism.

Donald Quataert effectively demonstrates that the belief that a woman's primary role was to be a wife and mother did not prevent Ottoman women from being a major part of the labor force in manufacturing during the nineteenth century. Similarly, Erika Friedl and Mary Hegland illustrate the important political and economic roles of rural Iranian women. Both authors maintain, however, that the relative freedom and contributions of women have been constricted by the ideological changes in postrevolutionary Iran.

The dynamics of social change in the modern Arab world are documented through a series of uniformly well-written essays that offer engaging and insightful studies of women affirming and empowering themselves—manipulating cultural symbols and contexts, pursuing careers, forging new marital arrangements, and pressing for an expansion of social rights and roles. Taken as a whole, we can see how literature, census data, law, history, and politics can be effectively mined to reveal the realities of women's lives, which include but also transcend the boundaries of sacred texts and traditional norms. All highlight the complexity (and inadequacy) of the term "patriarchy" and challenge images of passive, submissive, powerless Muslim women. Judith Tucker's skillful use of legal documents reveals the complexity of gender roles and affective family relationships in nineteenth-century Nablus. Julia Clancy-Smith shows how a woman, despite male dominance and French colonial interference, was able to succeed her father as head of a Sufi center. Baron documents the gradual transformation of the marriage ideal and of marriage laws in Egypt. Virginia Danielson's study of female singers (among them Um Kalthum) who asserted their right to manage their careers and own commercial enterprises is complemented by Cynthia Nelson's fascinating exploration of interpretive issues (of the subject as well as the biographer) that she has encountered in writing a biography of Doria Shafiq, a prominent Egyptian intellectual and feminist.

This collection makes an important contribution to Middle East scholarship. Readers come away indebted to many of its contributors for what they have

learned, as well as with a host of additional questions. For example, given the acknowledged impact of contemporary Islamic revivalism, other than posing it as a challenge to modern reforms, how effective and representative are Islamic women's organizations as compared to more secular-oriented groups and leaders such as those studied in this volume? How significant is the return to Islamic dress among educated women? Is there a new alternative elite, modern but Islamically oriented, emerging and what are its implications for the future?

More than a decade ago Keddie and Lois Beck edited an early and much-needed volume, *Women in the Muslim World* (1978). Now Keddie and Baron's volume appears at an equally important juncture in women's studies and will no doubt have a significant impact.

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JOEL GORDON. *Nasser's Blessed Movement: Egypt's Free Officers and the July Revolution*. (Studies in Middle Eastern History.) New York: Oxford University Press. 1992. Pp. vii, 254. \$35.00.

Joel Gordon's important book will likely become the standard work on the early years of military rule in Egypt. Through memoir materials, government documents, newspaper reports, and interviews of those who participated in the movement, the author offers a carefully researched and original view of the period.

The work first explores the decaying prerevolutionary period of quasi-liberal parliamentarianism. As Gordon argues, the failure of liberalism in the late 1940s was so dramatic that it fostered, even among liberal intellectuals, a tolerance of antidemocratic measures, measures that would later become synonymous with military rule. It was at this time of heightened political activity in Egypt by groups as diverse as the Muslim Brotherhood, local communists, socialists, members of the left wing of the Wafd Party, workers, and students, that the Free Officers emerged clandestinely as a politically conscious organization within the military. In contrast to these other highly vocal and sometimes tendentious groups, the Free Officers held few doctrinaire ideological positions. They were anti-imperialists, nationalists, and social reformists, and, despite some links with both the Muslim Brotherhood and the communists, they were fiercely independent of the competing political forces of the time.

For Egyptians long tired of the liberal status quo, many thought the coup would bring a "blessing" to Egypt, since the Officers had promised to end inefficient parliamentarianism, oust the disgraced monarch, and terminate political corruption and elitism. This generally positive public reaction was somewhat remarkable because the Officers had no clear vision

of what they wanted to achieve; except in the most abstract terms. Gordon quite rightly points out that in the first six months of military rule, the Officers chose indirect rule with the sole intention of accomplishing a "housecleaning." Adopting a moderate posture, they did not undertake either a social or a political revolution generally because they did not envision themselves as politicians. In time, however, they became more aggressive, clearer in their resolve to overhaul Egyptian politics and society, remove the pashas from power, and narrow the gap between rich and poor. The coup would become a revolution.

After overcoming several internal threats to military control, the Officers abolished political parties, promulgated land reform, and sought constitutional reform. But very early on, the Officers realized that they could not achieve their goals or ever be secure in their power until they gained popular support. Despite the general acceptance of military rule in the immediate aftermath of the coup, the Officers were never able, through all their years in power, to secure widespread approval or popularity. The Officers were feared (mainly because they were capable of unspeakable brutality to their opponents) and tolerated, but they were never loved.

Gordon's book includes a particularly interesting account of British and American relations to the Officers at the time of the coup and in its earliest phases. Unlike other researchers who have come before him and have written of Western antipathy to the military, Gordon corrects the view by documenting sometimes close and encouraging contacts between the two sides. In the end, he points out that while Nasser and his lieutenants brought national independence to Egypt, some economic development, and a high level of respect in Arab, African, and Third World circles, it was at the expense of parliamentarianism and political openness in Egypt.

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## AFRICA

LELAND V. BELL. *Mental and Social Disorder in Sub-Saharan Africa: The Case of Sierra Leone, 1787-1990*. (Contributions in Afro-American and African Studies, number 147.) New York: Greenwood. 1991. Pp. xi, 206. \$45.00.

Historical study of mental illness and the care of the insane, a growing field in North America and Western Europe, is relatively underdeveloped in many other parts of the world. For Africa there is almost nothing beyond a few articles and brief mention in general works on medical history. Leland V. Bell's welcome account of the Kissy Mental Hospital in Freetown, Sierra Leone, breaks new ground.

The Kissy Asylum/Hospital was established in 1844 by the British and served Britain's other West African

territories until late in the century. It remains the only mental hospital in the country. Kissy was founded as an all-purpose asylum, with a special mission to accommodate bizarre and violent persons who could not be controlled by family members and who did not belong in jail. Custodial care has remained a basic and, as patient case histories indicate, essential function.

Kissy existed in a state of penury during most of the colonial period; a backwater in the under-funded health system of a poor colony. Sierra Leone's miserable economic performance since independence has made conditions generally worse, as Kissy has had to serve a larger urban population and a growing number of patients from the hinterland. Until recently, the institution has rarely been managed by a trained psychiatrist. It was generally administered by a doctor whose primary concern was to provide good physical care in a humane environment. This, as Bell shows, was usually accomplished. Light work, a structured milieu, and simple discussion were the major therapeutic techniques. Some patients were cured or at least made well enough to be released to their families. In recent decades, electro-convulsive therapy, new drugs, and supervised community release have been employed with varying degrees of success. One innovation, nicely described by the author, has been cooperation with "traditional" healers, somewhat along the lines pioneered by Dr. T. A. Lambo at the famous Aro Hospital in Nigeria.

This book is in part an institutional history. As such it is generally successful, but there should have been more discussion of the staff, physical facilities, and relationships between the doctors in charge and the rest of the medical community. Photographs of facilities would have been valuable, and graphs of patient loads, staff, and budgets would have been helpful for comparative purposes. Numerous case histories are presented, many quite moving. Young men have always been overrepresented in the patient population and both the diagnosis of clinical depression and the proportion of substance abusers have risen sharply in recent years. Discussions of changing concepts of mental illness in Africa are illuminating and well grounded in the scientific literature.

This book, based on the government and Kissy Hospital archives and on local interviews, is a solid contribution to modern African history that touches on many aspects of socioeconomic and health change. It should also appeal to scholars interested in mental health in other times and places.

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AKINTOLA J. G. WYSE. *H. C. Bankole-Bright and Politics in Colonial Sierra Leone, 1919-1958*. (African Studies Series, number 64.) New York: Cambridge University



Press, with the cooperation of the African Studies Centre, Cambridge, England. 1990. Pp. xii, 278. \$49.50.

Written by Akintola J. G. Wyse, one of Sierra Leone's most prolific historians, this book has a double revisionist agenda: "to tell the truth about" and "repair the neglect with which history has treated" H. C. Bankole-Bright, a twentieth-century Krio political leader of Sierra Leone's preindependence era, and to contest "views which perceive the Krios as co-regents of the British colonial government and 'maximum participants' in the colonial system" (p. ix). In seeking to place Bankole-Bright "among our nationalist heroes" (p. 180), Wyse thus attempts to challenge historians and contemporaries of Bankole-Bright—persons who recognized "Bankie" for his early contributions to anticolonial politics, but who also noted his transformation in the 1930s and 1940s into an increasingly reactionary politician, more interested in safeguarding his own privileged position within the colonial setting than in bringing about a radical transformation of the colonial relationship.

In spite of these clearly stipulated revisionist goals, however, Wyse's book will disappoint anyone seeking fresh insights into the life-history and activities of Bright, or into the modern history of Sierra Leone's Creole society. Indeed, despite Wyse's professed use of "oral evidence to provide the background, the human angle, not obtainable from official records" (p. x), Bright, as a person capable of change over time, as a fleshed-out living being capable of self-doubt, self-criticism, affection, generosity, pettiness, and other human strengths and foibles, appears nowhere between the book's covers. Methodologically, Wyse's use of oral evidence (much of it from Bright's sister and niece, two not very disinterested witnesses) lacks sophistication. He seems uninformed about the ongoing theoretical discussions concerning the constructed nature of oral history—oblivious to the consensus among oral historians that oral testimony, like all else associated with memory, is constructed from the perspective of the present. We do learn that, among Bright's "good and attractive qualities," "he was a good dresser, neat and fashionable," that his ability as a public speaker was "peerless," and that he had the "indomitable courage" to "say 'Boo' to the whiteman" (pp. 180–81). We find out again—for this is certainly not news within the published historical literature on Sierra Leone—that Bright "contributed to journalism as a medium of criticism," and that his "fight against officialdom and the defense of his people were major contributions to the growth of nationalism and political awareness" (pp. 182–83).

Yet while Bright contributed to the growth of nationalism and, no doubt, was an occasional thistle, perhaps even a thorn, in the backside of the colonial administration in the interwar years, it is important to indicate that neither thistle nor thorn do inevitably a nationalist make. Bright certainly was a defender of

"the race" and spoke up strongly when he perceived racial discrimination; he was also a consistent voice in support of the aspirations of "aristo" Africans like himself (pp. 7, 12) to participate more fully and equitably in the professions and political institutions in colonial Sierra Leone. As Wise himself points out, however, Bright was "an elitist" who disliked the hoi polloi, distrusted democracy, and "believed in the imperial mission of Britain to Africa"; he was a "loyal subject" who admired British institutions and customs (pp. 8, 60, 119, 127). Some might therefore label Bright a "black nationalist"; others might even concur with assessments viewing him as a "Creole nationalist." But given Bright's role in the late 1930s in helping British colonial authorities to crush the West African Youth League, the popular multiethnic and radical nationalist organization led by I. T. A. Wallace-Johnson, it would be difficult to sustain an argument that he was a person wishing to bridge the various divisions within Sierra Leone society in an effort to seek independence from colonial rule.

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JULIUS A. AMIN. *The Peace Corps in Cameroon*. Kent, Ohio: Kent State University Press. 1992. Pp. viii, 240. \$32.00.

This case study by Julius A. Amin focuses on the Peace Corps programs in secondary education and community development in West Cameroon between 1962 and 1966. It examines the training of the volunteers at Ohio University during the summers of 1962–64 and follows them through their two-year terms in Cameroon until 1966. It assesses the generally positive results of their efforts and compares them with the less-successful programs in Ghana and Guinea during the same period. A program at Oberlin College for preparing secondary teachers of English for East Cameroon is also briefly treated.

Amin is a former student of the Peace Corps volunteers. He bases his study on previously unused primary sources, including the letters and diaries of returned volunteers, fifty of whom completed a questionnaire for him. He also interviewed the personnel who prepared the volunteers and supervised their work, as well as the Cameroonians who worked with them or knew them well. The result is a vivid portrait, enhanced by colorful details and apt quotations, of one chapter in the history of a noble experiment that enriched the lives of both Africans and Americans.

Amin shows how the presence of the volunteers permitted the rapid expansion of the secondary schools in West Cameroon after independence and reunification with East Cameroon in October 1961. It thereby preserved the English language and Anglo-Saxon heritage at a time when the more numerous East Cameroonians under Ahmadou Ahidjo were hoping to replace them with French influence.



Community development programs, to which smaller numbers of Peace Corps volunteers were assigned, produced some achievements through fencing in farm animals, improving sanitation, and creating corn mill societies that reduced women's labor. But lack of expertise among the volunteers, most of whom had just graduated from college, along with ineffective support from the Cameroonian government, limited their success.

At other levels, the volunteers had difficulty explaining American democracy in light of the Civil Rights movement and the anti-Vietnam War protests. But the volunteers initiated a degree of social mingling between races never before experienced, which won Americans as individuals many Cameroonian friends. Through their service the volunteers developed a better understanding of African problems and a greater concern for human well-being.

The study further reveals how the Peace Corps, which originated as an imaginative effort to prevent the spread of communism, quickly acquired a bureaucracy in Washington whose arbitrariness so damaged the effectiveness of the training program for West Cameroon that Ohio University withdrew after 1964.

Although Amin's work has many merits, its value is lessened by failure to discuss systematically the long-term effects of British and French policies during the periods of mandate and trusteeship, as was done for the effects of German rule (1884–1914). As a result, the reader receives an unclear and incomplete view of the complex problems facing Cameroon during the 1960s that form the context for Peace Corps work. A more specific lapse concerns conflicting statements, which are left unresolved, about the effectiveness of the volunteers at Baptist Teacher Training College (pp. 117, 123, 168–69).

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BIRGITTA LARSSON. *Conversion to Greater Freedom? Women, Church and Social Change in North-Western Tanzania under Colonial Rule*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 162.) Uppsala, Sweden: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1991. Pp. 230. 156 KR.

The Haya of northwest Tanzania inhabit the region west of Lake Victoria. In geography, culture, and language they are closer to the peoples of Rwanda and southern Uganda than to those of the nation to which they belong. As a result, the Haya have received short shrift in many histories of modern Tanzania, despite their early conversion to Christianity, their early involvement in cash crop production, and the importance of Haya migrants, male and female, to East African cities. Birgitta Larsson's dissertation is

the first monograph to look exclusively at the Haya in colonial times. While it may not be as theoretically sophisticated as some work on African women, its contribution to East African history—women's and men's—is enormous.

Larsson locates much of her history in missionary sources, Catholic and Protestant. Although she often brackets her arguments about whether women should be considered pawns in male strategies or agents in their own right, she has no difficulty in showing how Christianity was both a system of belief for Haya women converts and a social strategy used to hold male power, itself constantly shifting between husbands and fathers, at arm's length. Her data is frequently stunning; it places the Haya in the politics of colonial Tanganyika, giving us a new sense of regional history.

The book's chapter on venereal disease shows how colonial obsessions with African birthrates became in Buhaya a fixation on the perils of African agricultural wealth. Officials claimed that the profits from coffee made Haya men profligate, and their debaucheries spread syphilis to their wives. Reports were commissioned not to ameliorate the conditions among the Haya, but to keep other peoples in equilibrium, not too rich but not too poor. Larsson has a good eye for the meaning of wealth to the Haya, however. Her chapter on prostitution is not only based on a number of interviews with former prostitutes, but presents remarkable documents by prostitutes (or their solicitors) who applied for permission to leave their rural homelands and, once in Dar es Salaam or Nairobi, protested policies to curtail women's travel or formed welfare associations that refused to merge with Haya men's associations. Women's associations were bankrolled by prostitutes, and they did not want to relinquish control to those who did not contribute as much money. Her chapter on *balokole*, the Protestant revival movement lasting from the 1930s to the 1970s that restructured private life far more thoroughly than missionaries did, shows how African marriages and family structures were by no means fixed, but contested, subverted, and reconstructed by Africans themselves. Now that policy makers have once again descended on Buhaya—this time because of AIDS—this chapter is essential reading.

Students of East African history will not find anything surprising in Larsson's history of cash crop production or capitalist development in Tanzania, and, like me, they will probably wish that she discussed her chapters in relationship to each other more. To what extent, for example, did colonial discourses about the perils of wealth and the evils of venereal disease enhance *balokole*'s vision of the African family? How did that vision influence Haya male opposition to Haya prostitution? Nevertheless, this book covers some important ground, bringing to light new documents and old obsessions, all the while

placing women's experiences at the center of Haya colonial history.

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CLIFTON C. CRAIS. *White Supremacy and Black Resistance in Pre-Industrial South Africa: The Making of the Colonial Order in the Eastern Cape, 1770–1865*. (African Studies Series, number 72.) New York: Cambridge University Press. 1992. Pp. xvi, 284. \$59.95.

Clifton C. Crais contends that in the eastern Cape "the first half of the nineteenth century witnessed not only the creation of classes and 'races,' but also the creation of a 'structure of thought'" (p. 3). Dealing with a subject that Noel Mostert has expansively treated (*Frontiers: The Epic of South Africa's Creation and the Tragedy of the Xhosa People* [1992]), Crais adds many fresh insights in a quarter the space. In particular, he illuminates the changing goals and perceptions of the diverse participants in the process that resulted in the conquest of the African peoples in the region (hunter-gatherers, herders, and mixed farmers) and their incorporation in a colonial state dominated by white settlers.

Adapting Michel Foucault, he describes how, after conquering the Cape from the Dutch, the British brought a new hegemonic notion of the rule of law and equality before the law. Emancipating the Khoikhoi from their serf-like status (1828) and freeing the slaves (1834), they robbed the Dutch colonists of the patriarchal basis of their identity and created the state of mind that led to the "great trek." Crais also provides a fresh analysis of the evolution of the stratified "coloured" community, increasingly conscious of its common interests, from Khoikhoi, slave, and white antecedents; and he examines the successes and failures of the Kat River Settlement, which was founded for coloured peasants in 1829 and collapsed under colonial pressures in the 1850s.

Crais demonstrates that the leaders among the British settlers who arrived in the area in 1820 soon abandoned their original intention to create "a stable but stratified rural society" of whites (p. 90). Instead, they became dependent on the labor of Africans, whom they treated as barbarians. They gained control of the local administration in the 1830s, and after 1854 they cooperated with white farmers and merchants in the western Cape in enacting a series of laws that went a long way toward reestablishing the controls over black labor they had lost in 1828 and 1834.

Crais depicts the British and the Xhosa as "two societies with radically different ways of perceiving the world around them" (p. 100). Drawing attention to the clash of cultural symbols and practices concerning time and space, he notes the significance of the mission bell and contrasts the round African huts with the rectangular buildings of the missions and the

colonists. After summarizing the series of wars that culminated in the especially brutal campaigns of 1850–53, which were followed by the spread of a disastrous cattle disease, he presents in a controversial passage the Xhosa cattle-killing as "a popular uprising," in which the Xhosa "rejected the colonial order in the symbolic recreation of community" (p. 210). Finally, he describes the creation among the conquered Xhosa of new, sharply stratified classes: well-to-do peasants, tenant farmers, sharecroppers, and rural and urban proletarians. He contends that, despite these tragedies, the colonial state did not succeed in dominating the minds of the conquered.

As Crais points out, the complex process that he has analyzed has wide significance because in later years white settlers and officials in other parts of South and East Africa consciously borrowed and applied ideas and practices that had germinated in the eastern Cape. In this interesting book, Crais strains conspicuously after fashionable postmodern ideas and makes excessive claims to originality.

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RICHARD MENDELSON. *Sammy Marks: "The Uncrowned King of the Transvaal."* Athens: Ohio University Press or David Philip, Cape Town, with the cooperation of Jewish Publications—Cape Town, South Africa. 1991. Pp. x, 304.

This is a long-awaited study by Richard Mendelsohn of a pivotal figure in the economic history of South Africa. Sammy Marks arrived in the Cape Colony in 1868, having left Lithuania six years before and after having failed to improve his standard of living in England. His arrival coincided with the discovery of diamonds and from then until his death half a century later he was to be an integral part of South Africa's mineral revolution. Initially an itinerant dealer (*smouse*) in German knives, Marks became by turn, and often concurrently, a diamond merchant and claimholder in the Kimberley mines (and a person often suspected of illicit dealing in gemstones), the owner of some of South Africa's major collieries, the overseer of vast agricultural estates, the founder of the country's first factory (a distillery), and a major developer of the South African steel industry. Indeed, he was the country's primary entrepreneur, striving to capture some of the wealth generated by the diamond and gold industries by attempting to monopolize all industries ancillary to mining. Although he failed ultimately to make a fortune on the order of those of Cecil Rhodes and Alfred Beit, it is a testament to the acuity of his vision that most of the industries he started were deemed significant enough to South Africa's economy that they were eventually taken over by the state or by the huge mining conglomerate, the Anglo American Corporation. Marks's career was so wide-ranging that study of it takes the

historian into virtually every niche of South Africa's emergence as a modern industrial state.

Writing a life of Sammy Marks proved to be something of a "biographer's dream" (p. ix). In 1984 a vast quantity of documents previously known only to his heirs—including 20,000 pages of Marks's personal and business correspondence detailing practically every year of his life—was deposited suddenly in the University of Cape Town library. Access to this material was limited to Mendelsohn, who was commissioned by Marks's heirs to write the biography, with "very generous financial support" (p. ix) provided by the Anglo American Chairman's Fund. Using this material as the basic resource for his study, Mendelsohn has also ranged widely in other archives to produce a meticulous and exhaustively researched book.

Yet while the book provides an enormous amount of detail about Marks, it is much less illuminating about his family, his contemporaries, or the massive economic transformation in which he played so important a role. Perhaps because of the quantity and richness of Marks's papers Mendelsohn stays close to the man and his documents; if Marks comments on something we hear about it; if he does not, we do not. Thus, there is relatively little about Marks's family and their complex and contradictory notions about their Jewishness (as distinct from Sammy Marks's own concerns), although the few glimpses we get suggest that this would have been a fascinating topic for extended discussion (see especially pp. 199–201). The examination of Marks's business exploits is phrased in the language of upward mobility and accomplishment while doing little to explain why his contemporaries all assumed that he engaged in shady practices. There is no discussion of Marks the employer, even though his various businesses used the labor of hundreds of white and thousands of black men and women. Africans do not appear in the book. In short, what we have is a rather conventional biography and business history about a man long considered most unconventional.

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#### ASIA

JENNIFER W. JAY. *A Change in Dynasties: Loyalty in Thirteenth-Century China*. (Studies on East Asia, number 18.) Bellingham, Wash.: Center for East Asian Studies, Western Washington University. 1991. Pp. xiv, 309. \$35.00.

The idea that an official of one ruling house should not serve another presents a complex set of issues for historians of late imperial China. Loyalists who died resisting new regimes earned the praise of subsequent generations of scholar-officials. Their writings survived as sources while the writings of less virtuous men passed into obscurity.

The survivals introduced into the historical record a bias supported by the power of the idea. Successor dynasties demanded loyalty of their own officials, and rebels claimed roots in the loyalist martyrs. Jennifer W. Jay has examined the biographies of 300 Song loyalists in an attempt to distinguish fact from fiction in the 700-year-old record of their resistance to the Mongol Yuan conquest of southern China and the influence of that resistance on later generations of Chinese scholar-officials. The results are mixed.

The chief contribution of this book is the author's debunking of the myth of a widespread, unified, nationalistic Song loyalism in response to the Mongol invasion. In contrast she describes a broad spectrum of attitudes and behaviors among men of the official class who chose not to serve the Mongols immediately after their conquest of the southern Song in 1276. She begins by exposing the traditional praise of the patriotic martyr Wen Tianxiang and blame of the unsuccessful Song chief minister Jia Sidao as the work of myth makers. She then explains how local survivals and family traditions contributed to a cult of Song loyalism that lasted into the Ming. One key factor in this survival was the formation of small, unconnected groups of *yimin* (survivors) scattered about the empire. Jay shows how members of these groups adapted to Mongol rule, associated with Yuan officials, and changed their attitudes over time. She demonstrates that during the Yuan-Ming transition Yuan loyalists appealed to the same idea of loyalty while the Ming founders made little of Song loyalist patriotism. Finally, she argues that racist or nationalistic interpretations of Song loyalism only emerged in the seventeenth century with the much more widespread resistance to the Manchu conquest in the name of the native southern Ming.

Recent scholarship on Song, Yuan, and Ming history has already established many continuities through the period of Mongol rule, and few would disagree that Chinese ethnic nationalism was at most latent and probably not broadly distributed among scholar-officials even in the nineteenth century. Moreover, social historians have been arguing over the degree to which the scholar-official class shifted its base from court connections to local elite communities during the Song. It is unfortunate that such a careful study of individuals so strategically placed at this crucial juncture does not engage these broader social and political issues. The author's careful treatment of the influential loyalist literatus Zhou Mi confirms the effect of reunification under the Mongols on the development of art and literature, but the broad revision of the place of the loyalists in China's cultural history remains to be written.

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PIERRE-HENRI DURAND. *Lettres et pouvoirs: Un procès littéraire dans la Chine impériale*. (Civilisations et soci-

étés, number 84.) Paris: École de Hautes Études en Sciences Sociales. 1992. Pp. 468. 200 fr.

In 1713, the scholar Dai Mingshi was executed for sedition; many of his colleagues and their families were exiled or imprisoned. Dai had used Ming reign-titles in discussing the princes who had tried to maintain that dynasty in the south after the Manchu conquest and the foundation of the Qing dynasty (1644–1911). To alien rulers acutely sensitive to any slur against their legitimacy, such an act constituted high treason. Dai's offending works were burned and it was over a century before a carefully pruned collection was compiled for publication (under a pseudonym since Dai's name still remained a potent symbol of antidynastic sentiment and its repression). The case has sometimes been taken as indicative of the Kangxi emperor's decline in the later years of his reign (1662–1722). Pierre-Henri Durand's new study of Dai's life and untimely end does not overturn that view, but it locates the episode convincingly in the complicated factional disputes of the period.

This elegant and richly detailed book is divided into three parts. Part 1, "A Literati History," amounts to a composite biography of the protagonists in the case and their associates, many of whom were "post-loyalists" (p. 350) deeply ambivalent about serving the Qing. Apart from Dai himself, who was frustrated by his inability to live up to his own ideal of moral integrity, the author focuses particularly on Dai's friend and fellow-countryman Fang Bao who, imprisoned in 1713 for allegedly writing a preface for Dai, nonetheless survived to pursue a distinguished scholarly career. We gain a fine sense of the development of political and literary friendships and antagonisms among the men who gravitated toward the capital in search of advancement.

Part 2, "Politics," vividly places the case in the context of the leading political issues of the later Kangxi period, including the bitter succession struggle and the complex oppositions between Chinese and Manchus. The author discerns a pattern of increasing severity for Manchus and relative leniency for Chinese in imperial arbitration of ethnically charged conflicts at this time, although their ultimate treatment often was virtually indistinguishable. Part 3, "Scholars and History," assesses Dai's work and his role as a historian, focusing on the important use of history as an instrument of legitimacy. It connects Dai with Lü Liuliang, a scholar Dai admired whose anti-Manchu writings led in 1732, long after both were dead, to another wide-ranging literary prosecution. In a twist whose irony all recognized, Fang Bao, once condemned for his links to Dai, was called on to help discredit Lü's classical scholarship.

Durand has read widely in private and official sources, the majority in published form, and builds on others' work to cast fresh light on an unpleasant episode that had important and long-lasting reverberations. Given his explicit connection of Dai's case

with the succession struggle, he is perhaps insufficiently cautious about the *Veritable Records* that Kangxi's successor is known to have rigorously censored. All Qing scholars, and others interested in the power of the written word, will want to read this book.

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CRAIG CLUNAS. *Superfluous Things: Material Culture and Social Status in Early Modern China*. Urbana: University of Illinois Press. 1991. Pp. vii, 219. \$39.95.

In this bold and insightful study of the ideological dimension of material culture in seventeenth-century China, Craig Clunas establishes the importance of material consumption as an index of much larger historical processes. Students of early modern Europe as well as China will find its arguments both pertinent and compelling.

Beginning with a review of major Chinese treatises on the selection, ranking, and use of articles of elite consumption, Clunas moves on to a broad range of topics about "things," including culture-coded categories for things, specialized terminology for evaluating things, art collecting, the workings of the art market, and the social "anxieties" surrounding the acquisition of fine things. He furnishes copious details about the pricing, consumption, marketing, and distribution of luxury goods that will be appreciated by historians of Ming dynasty art and society. An appendix listing prices for selected works collected between 1560 and 1620 will be welcomed by many.

But this book is not a mere compendium of facts about the Ming economy. One of Clunas's major aims is to alert students of Europe to the history of material culture in China, "with its sometimes striking prefigurations of and parallels with early modern Europe" (p. 3). Having discovered significant parallels, Clunas is forced on several occasions to question some well-worn assumptions of sinological writing.

For instance, a common theme in traditional sinology is China's all-pervasive reverence for the past. Clunas fails to see the uniqueness here, noting the exceptional importance of classical culture for intellectuals in early modern Europe (pp. 91–92). China's xenophobic aversion to foreign cultures is another traditional saw, and this, too, Clunas reexamines on the basis of his documents. Most importantly, he questions the status of that materialist ethos which, supposedly, was unique to early modern Europe. Clunas suggests rather that Europe may even have lagged behind China in what he calls the "commodification of culture." He makes such observations not to make China more appealing to the West, but rather as "part of a project to *defamiliarize* notions of 'China' as the essential contrast to Europe" (p. 172).

Clunas is able to make meaningful comparisons because of his respect for European history. For



example, when he considers lists of high-status articles in seventeenth-century China, he compares them to similar lists in Italy. This represents a break from sinological studies inspired by the social sciences, where early periods in China typically are compared to something called "the West." Since "the West" is an ahistorical construct (eternally democratic, scientific, and individualistic), not surprisingly, few sinologists have found significant parallels between it and pre-modern China. Clunas's book eschews such abstractions and, along with some other recent publications (Colin Mackerras, *Western Images of China* [1989]; Martin Powers, *Art and Political Expression in Early China* [1991]), suggests the need for a revision of our familiar constructions of Chinese culture. As Clunas's book clearly shows, any reconstruction of our view of China will surely require a reassessment of ideas about Europe and the modern world.

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CYNTHIA J. BROKAW. *The Ledgers of Merit and Demerit: Social Change and Moral Order in Late Imperial China*. Princeton: Princeton University Press. 1991. Pp. x, 287. \$42.50.

Cynthia J. Brokaw begins her study by reviewing the early development and evolution of the concept of moral retribution from ancient China to the late empire and delineates the two chief views of fate that emerged: first, a system of merit and demerit that human agents can influence; and second, a framework of meaning beyond human influence. Through the efforts of sixteenth-century literati such as Yuan Huang, the chief example in Brokaw's study, the Chinese increasingly turned to merit accumulation in the moral realm as a system homologous with worldly material benefit. In her presentation of the seventeenth-century debate between moral purists, typically neo-Confucians, who thought fate beyond human influence, and those like Yuan, who favored promotion of merit accumulation among elites for success in civil examinations and among common folk for personal self-improvement in daily life, Brokaw guides us through the creation of a moral vision for those in late-Ming society aspiring to social mobility and material success.

According to Brokaw, such fluid aspirations, which were at first written into and later read out of the ledgers by an upwardly mobile elite, shifted in the late seventeenth century to a conservative ideology, whereby the records of merit and demerit became tracts depicting the paternalistic responsibilities of elites to maintain their leadership in local society and to guide the lower classes. In the eighteenth century, the era of social and moral confusion that had marked the late Ming had ended, yielding instead a social vision of money and moral capital that rein-

forced social stasis and promoted Confucian cultural hegemony. The ledgers were ubiquitous throughout elite and popular culture. Brokaw notes, however, how these moral tracts were appropriated differently by Confucian elites and popular folk. Differences in material resources between elites and nonelites did not prevent the sharing of moral visions in late-imperial China. Differences in practice, however, depended on the higher or lower location in the social hierarchy where people created and used the ledgers to measure their moral worth.

Brokaw's study of previously overlooked or undervalued Chinese ledgers of merit and demerit is an important contribution to both the intellectual and social history of late-imperial China. She successfully relates the forces of commercialization in Ming China to the changing nature of Confucianism during the sixteenth and seventeenth centuries. Her discussion of how an increasingly monetized economy was interpreted by gentry elites and assimilated into Confucian life by the eighteenth century is a major addition to our understanding of late-imperial intellectual life, enriching and modifying those earlier interpretations of Confucianism in Ming and Qing that were based on a formalist history of ideas approach, which overdetermined the role of neo-Confucian philosophy in Chinese cultural history. Brokaw deserves credit for bringing together into a coherent and convincing narrative the interdependent aspects of social change and moral order during the Ming-Qing transition. Moreover, her account moves from elite intellectual events to popular culture and mentality fluidly, revealing the historical context within which commoners and gentry had to redefine their moral ideals and the measures of those ideals during a time of remarkable social change.

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JOSEPH W. ESHERICK and MARY BACKUS RANKIN, editors. *Chinese Local Elites and Patterns of Dominance*. (Studies on China, number 11.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xvii. 450. \$55.00.

This volume, edited by Joseph W. Esherick and Mary Backus Rankin, is part of an influential series that resulted from workshops sponsored in the 1980s by the Joint Committee on Chinese Studies of the American Council of Learned Societies and Social Science Research Council. The essays here can be seen as emblematic of the welcome replacement of the state-oriented picture of a homogeneous "gentry" by a local perspective that emphasizes more diverse and contentious "elites" in the field of Chinese social history.

The construct of "the Chinese gentry," arising from a perceived similarity between the educated



landholding elites of early modern China and England, was given classic formulation by Chinese scholars working in the United States in the late 1950s and early 1960s. The more recent shift to the term "elites," influenced by Japanese, Chinese, and Western scholarship, reflects a greater appreciation of the variety and the flexibility of this ruling group and the complexity of Chinese society. The social stratum considered in this volume is in consequence far broader than the old scholar-officials, and includes all kinds of people (usually families) who "exercised dominance" in a variety of spheres. This book also illustrates how, out of a monolithic "Chinese society," historians have begun reconstructing the local societies that were the components of this empire.

If you are interested in keeping up with modern Chinese history, you should know the names of the eleven scholars whose work appears in this volume. Unfortunately, space does not permit more than a listing of them here. Late-imperial elites are treated in chapters by Timothy Brook on cultural hegemony in Ningbo (in the Ming and Qing), William Rowe on the elite families in Hanyang (from the Ming to Republican eras), and Madeleine Zelin on salt merchants in Sichuan (late Qing). Local elites in the late nineteenth and early twentieth centuries are described in essays by Lynda Bell (silk magnates in Wuxi), Keith Schoppa (local elites in Zhejiang), and Edward McCord (military families in Guizhou). There are five chapters on the elites of the early twentieth century: Lenore Barkan on new politics in Jiangsu, David Strand on urban leaders in Beijing, Rubie Watson on society in the Canton delta, Prasenjit Duara on North China villages, and Stephen Averill on the hill country of Jiangxi. Each author looks at the elites of one area—the size of a county or smaller—to see them as "actors in local systems" and to probe in detail the range of strategies and patterns of dominance manifested there over a substantial period of time.

The chapters span the fourteenth to twentieth centuries, but the center of gravity is in the complicated but important transition across the turn of the twentieth century, at and after the end of the Qing period. The volume thus makes very clear the foolishness of allowing the political divide represented by dynastic collapse in 1911 to impede the study of many other continuities and developments.

Because each essay is concerned with the fabric of local society in a different part of China, the reader is exposed in interesting detail to the concrete diversity of China. There is also a useful discussion in the introduction comparing the elites of the different regions of China. You will learn about drinking parties, lawsuits, financial institutions, new industries, urban associations, marriage networks, philanthropy, riots and militias, modern education, reputation and "face," chambers of commerce, corporate property, feuding, patronage, lineages, and landlordism. Although one misses the complicated hybrid world of

twentieth-century Shanghai, there is fortunately other work now underway on this subject by scholars not represented in this volume.

These are excellent essays, ably set in context by the substantive introductory and concluding chapters by the editors, who also organized the conference out of which this volume developed. They set out the general issues and then draw the book together with discussions of status and stratification in the imperial system, the varied resources and strategies used by local elites, the span and patterns of elite activity and dominance, changes in elites over time, and the relationship between elites and the state.

The book as a whole has the effect of replacing the picture of a conservative scholar-gentry class that had to be ousted by revolution with one of diverse ruling elites who were both hegemonic and adaptive. The editors stress the variety engendered by local conditions, the insecurity of status, and the resultant "flexible application of a broad repertoire of strategies [by elites] to preserve their positions" (p. 307), the continuity of behavioral patterns across time, and the adaptability of old elites to the wider modern world.

It is a reflection of the traditional biases of studies of Chinese society that religion is here, as in most books on modern China, underrated or overlooked as an element of cultural systems, symbols, and power. Historians of premodern China are finally "discovering" religion; it is time the modern historians did the same.

In this volume, furthermore, as in much other recent work on Chinese social history (including my own), the word "elite" is embraced enthusiastically and quite uncritically. A term that permits analysis of dominance without the language of class has obvious appeal (and is certainly an improvement over "gentry"), but it hardly comes free of cultural baggage. Should we not begin to worry about this?

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JEFFREY N. WASSERSTROM. *Student Protests in Twentieth-Century China: The View from Shanghai*. Stanford: Stanford University Press. 1991. Pp. ix, 428. \$45.00.

Students played a large part in the dramatic mass demonstrations during the republican period in China. Jeffrey N. Wasserstrom focuses on university students in Shanghai, concentrating on four main incidents: The May Fourth Movement of 1919, the May Thirtieth Movement of 1925, the movement to save China from Japanese aggression in 1931, and the congeries of struggles from late 1945 to 1947. He draws comparisons with Western student movements and ends with a sensibly cautious chapter pointing out similarities and differences in the renewed Chinese student protests of the 1980s.

The book somewhat uneasily combines surveys of several well-known protest movements with a pene-

trating exploration of student cultural practice. Segregation of information into chapters on political contexts, case studies, and thematic analyses causes some repetition and fragmentation. Such caveats aside, the work offers valuable insights into how the student message was conveyed.

Wasserstrom draws on anthropological concepts of political theater in Victor Turner's analysis of collective actions and Clifford Geertz's depiction of Bali as a theater state sustained by symbolic ritual. He also derives inspiration from studies by European social historians such as E. P. Thompson and Natalie Zemon Davis, showing how cultural symbolism might be used either to maintain upper-class hegemony or to challenge it from below. The thematic chapters are infused with the language of theatrical performance. Student players have repertoires, follow scripts, communicate with audiences, and use props such as bloody clothing. A secondary metaphor of protests as battles with tactics, marches, and strikes comes from the militant perceptions of the students themselves.

Most broadly, Wasserstrom is talking about political culture, but in the sense of fluid, varied, and changing patterns of practice in contrast to previous analyses that postulate an authoritarian, state-centered Chinese culture underlain by psychological patterns of dependency. He treats culture as symbolic practice and communication as discourse in exploring connections between revolutionary politics and culture and the transformation of power relations. This raises difficult issues of how practices are transmitted over time or shifted from one sphere of action to another.

Repertoire and script are key words in the analysis of how practices are translated into general cultural understandings that guide, but do not altogether determine, new actions. Such transmissions are most easily demonstrated over relatively short periods. Experiences acquired in campus life provided students with organizational skills useful for demonstrations, and students consciously looked to recent models like the May Fourth Movement, although each new demonstration reflected changes in the political situation.

Wasserstrom raises the question of longer, persistent cultural practices in his discussion of how students communicated with semiliterate workers. He notes that Chinese scholars had addressed similar problems before the twentieth century and that different levels of society shared a fairly unified system of values. Some of his pre-twentieth-century precedents, like simplified morality books or popular opera, seem more convincing than others, like the largely ignored local lectures on the Kangxi Emperor's Sacred Edict. Wasserstrom appears to postulate unrelated but analogous examples that lived on in habits or assumptions about how things were done. This perspective is suggestive but hard to formulate precisely. One also wonders whether the students

communicated as successfully with workers as Wasserstrom suggests.

Wasserstrom's ultimate purpose is to explain power relations, which he undertakes with attention to both general contexts and specific circumstances. Symbolic analysis certainly does not explain all politics, but it is effective in helping to understand why demonstrators might destabilize much more powerful governments. Wasserstrom combines Geertz's stress on the ruling elite's use of ceremonial symbols to buttress consensual charismatic authority, Antonio Gramsci's insight that cultural hegemony must be constantly asserted and reimposed, and Paul Lucas's insistence that popular demonstrations are also rituals aimed at gaining rather than defending power. Student organization modeled on bureaucracies and student directives mimicking government proclamations did not indicate state cultural dominance. Instead, these acts can be seen as symbolic, mocking appropriations of state ceremonies and procedures, implying that ruling elites did not truly speak for everyone. Such denigration of central symbols is particularly effective against authoritarian governments that depend on a monopoly of ritual to indicate that they represent the people. More detailed analysis of texts may be needed to explicate these ideas fully, but such insights make this book important to understanding republican politics. Wasserstrom's interpretations open up further possibilities for comparative study and may perhaps also be applied to traditional political relationships in imperial China.

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MING K. CHAN and ARIF DIRLIK. *Schools into Fields and Factories: Anarchists, the Guomindang, and the National Labor University in Shanghai, 1927-1932*. Durham, N.C.: Duke University Press. 1991. Pp. 339. \$47.50.

The tragic story of the National Labor University's rise and fall is a fascinating one that touches on nearly all of the central themes of the first years of Guomindang rule in China. Founded during a high point of revolutionary enthusiasm by Guomindang-supported visionaries (many of whom were anarchists), the National Labor University (or Laoda) was designed to serve as a model institution of "genuinely popular education" (p. 1). But the idealism of 1927 soon gave way to disillusionment as the founders proved unable to put many of their ideas into practice, the government grew increasingly hostile to anarchist beliefs, key university personnel were demoted, and the school became enmeshed in "a web of bitter conflicts" (p. 216). Thus, even before the Japanese invasion of 1932 dealt Laoda its final blow, there was evidence that "it had fallen short of the goals its founders had envisaged" (p. 1).

As interesting as this story is, Ming K. Chan and Arif Dirlik's ambitious book—the title of which comes

from Laoda's slogan, "Turn schools into fields and factories, fields and factories into schools" (p. 1)—is much more than a carefully reconstructed account of the brief life of an unusual university. In fact, only two of the work's nine chapters (3 and 8) are devoted to this narrative. The rest of the book can be divided into two parts: four middle chapters that present a wealth of detail on specific topics relating to campus life at the school; and three chapters (1, 2, and 9) that use the case of Laoda as the starting point for theoretical discussions of the Chinese discourse on labor education.

When taken as a whole, the four middle chapters provide a wonderfully comprehensive account of how a university in the Republican era (1912–49) actually functioned, an account that can usefully be read in tandem with recent books on modern Chinese education history (most notably Yeh Wen-hsin's *The Alienated Academy: Culture and Politics in Republican China, 1919–1937* [1990]) that compare and contrast different institutions. The amount of detail Chan and Dirlik provide in these chapters is impressive, especially since (in contrast to various other schools) no major Chinese-language *xiaoshi* (campus history) currently exists for Laoda. There are times, however, when the detail becomes overwhelming and distracting; discussions of some issues (such as course offerings and personnel shifts) could have been summarized more effectively.

A more significant problem with the book's middle chapters has to do with the question of Laoda's uniqueness. To their credit, in chapter 3 Chan and Dirlik emphasize the links (including overlapping personnel) between Laoda and other Shanghai universities. In subsequent chapters, however, the authors go a bit too far in emphasizing Laoda's distinctiveness. For example, their claim that the "scope and function of Laoda's student organs were extraordinary" (p. 165) may be justified, but many (although admittedly not all) of the specific organizational phenomena they describe on the pages immediately preceding and following this statement had close counterparts (if not exact equivalents) on other campuses. In addition, while Laoda's formal emphasis on labor education made it a very different school than Shanghai University (or Shangda), the institution whose facilities the school inherited in 1927, it is problematic to make too much of this contrast. After all, Shangda was famous for producing (and encouraging the pursuits of) students who engaged in just the sort of bridge-building between the academic and laboring worlds that Laoda's founders stressed. The activists Liu Hua (a *bangong banxue* or "half-work, half-study" youth who was a leader of both Shangda student groups and local labor organizations) and Yang Zhihua (who put her academic interest in sociology to use organizing textile workers while attending Shangda) are but two famous cases in point.

Chan and Dirlik's set of theoretical chapters, which trace the theme of labor education through various stages of the Chinese Revolution, comprise perhaps the most important part of their monograph. Unfortunately, it is also the part to which it is hardest to do justice in a brief review. Suffice it to say that the authors provide their readers with a great deal of valuable food for thought. Although I take issue with specific features of their passionately argued (and often insightful) analysis, I will never look at pronouncements during either the New Culture Movement or Cultural Revolution on combining labor and book learning in quite the same way again.

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RICHARD CURT KRAUS. *Brushes with Power: Modern Politics and the Chinese Art of Calligraphy*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xii, 208. \$24.95.

Once again Richard Curt Kraus has gone where no political scientist or historian has gone before. In his 1989 book, *Pianos and Politics in China*, he examined the question of imported Western culture and its social-political implications in modern China. Now he tackles the even more difficult issue of the continuation and transformation of China's traditional elite culture in the modern revolutionary era.

Kraus admits that he is "not an art historian, nor a literary critic, nor a cultural and social historian" (p. xi). As a political scientist he is primarily interested in political questions, particularly the cultural manifestations of social status and political power. Historians, however, should not be deterred from delving into this short, impressionistic, but highly original and extremely suggestive book. In fact, social and political historians, even more than art historians, may find it especially rewarding for the examination of how a key element of Chinese tradition has survived, adapted, and changed in the twentieth century.

The first third of the book provides a general discussion of "The Institution of Calligraphy in Imperial China." Some of this will be redundant for specialists but it makes the subject available to more general readers. The central section then shows how calligraphy as both "metaphor for China's cultural legacy" (p. 159) and basic instrument of communication fared in the Communist revolution. Surviving early iconoclastic attacks and suspicion about its elite associations, calligraphy became under that enthusiastic calligrapher and poet, Mao Zedong, an important means of personal and factional communication among the new rulers. Here Kraus is particularly good in drawing out the political implications of cultural patronage in the Cultural Revolution era

(1966–76) and its aftermath. But beyond these interesting examples of the expropriation of traditional cultural prestige by these cultural revolutionaries, Kraus demonstrates how, especially for Mao, calligraphy became an important tool for mass mobilization and communication. The unhappy fate of Mao's successor, Hua Guofeng, is also neatly shown through his failed attempt to imprint the authority of his handwriting on China's public monuments and mass media. The short concluding section on "Postrevolutionary Calligraphy" is more tentative about the significance of trends in the 1980s such as the official rehabilitation of old high culture, the emergence of professional calligraphers on the new art market, and the decline of dexterity with the writing brush both among the general population and the new generation of political leaders.

In his final chapter Kraus addresses the larger question of the survival and significance of tradition in modern China. Borrowing from Joseph Levenson, he suggests that tradition has neither disappeared nor survived intact. Rather, some elements that are useful to a new situation survive, such as the cultural and political use of calligraphy, although changing their form and overall significance. This seems to be about as clear and sensible an answer as is now possible. More studies such as this that scrutinize specific elements of cultural tradition in their historical context will help refine this answer while raising new questions about tradition, modernity, and revolution.

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JENNIFER ROBERTSON. *Native and Newcomer: Making and Remaking a Japanese City*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xvii, 325.

In the past decade or so, historians and social scientists, especially anthropologists, have developed a sophisticated appreciation of how the poetics and politics of tradition operate in past and present societies. The twentieth-century vicissitudes and present predicament of Japan make it a promising site for such a study and enhance the significance of this book for a wide audience.

The subject of Jennifer Robertson's fascinating study is not the making of Kodaira, a fast-growing suburban city in the Tokyo metropolitan region. Rather, it is the making of "Kodaira," an imagined projection of that city. "Kodaira" is "Native-place" Kodaira (*Furusato* Kodaira), and the process by which this trope has been represented and enacted in the past twenty-five years is part of a national and highly political promotion of local community-building that has emphasized nostalgic renditions and misrememberings of past solidarities.

In the making of Native-place Kodaira, "reclama-

tion" has proven to be a potent metaphor because it links the circumstances of earliest village settlement in the seventeenth century with post-World War II urbanization, and now with the 1980s reclamation from urban sprawl. Robertson's central proposition is that what constitutes Native-place Kodaira is a "dialectic of native and newcomer." There are only a handful of natives among the 150,000 residents, yet it is a sanitized version of the natives' past on which the city administration bases its campaigns to "intertwine 150,000 hearts."

Several ironies are joined in this clash of roots and rootlessness. The images of agrarian village cooperation and pastoral sentiments play on and to the natives, but by extending them as motifs for the whole city the images threaten to marginalize these very natives. Thus, the natives in turn are driven to keep their shrine parishes exclusive, to organize a local history society that keeps newcomers (and visiting anthropologists) at a polite distance, and so on. Newcomers, for their part, are encouraged to feel inside and attached, but they are then given few opportunities to participate fully, as in the annual city parade. And, finally, the legitimacy of this imagined moral community is its historicity—and yet it bears only the most distorted connection to Kodaira of the past.

The "literary portrait" that Robertson aims for evocatively elicits the rhetoric and performances especially of natives' efforts to reclaim their place in this suburban city. Just how an appreciation of "Kodaira" helps in understanding Kodaira, at least for the lives of the vast majority of newcomer residents, is less obvious. One may suppose that there is a political economy as well as a cultural poetics to native place-making, but individual political careers, business interests, citizen demands for green space, and local administrative struggles to capture resources from higher levels of government remain tangential to the book's main focus.

In the end, too much is made of the dialectic of native and newcomer. To be sure, this is paradoxical (p. 109), ironic (p. 191), even contradictory (p. 148), but it is hardly surprising or puzzling. Probing the dialectics of identity and difference is a hallmark of much contemporary analysis, including insightful work by Japan scholars such as Theodore Bestor, Dorinne Kondo, Eyal Ben-Ari, Brian Moeran, and Marilyn Ivy. Unfortunately, Robertson ignores all of them, for it is not the novelty of her project but rather her book's place in this new scholarship that gives it value. Robertson has written an instructive study for anyone concerned with the condensation of past, present, and future into a highly contentious politics of heritage that so often shapes modern identities of place, ethnicity, and nation.

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HUE-TAM HO TAI. *Radicalism and the Origins of the Vietnamese Revolution*. Cambridge: Harvard University Press. 1992. Pp. x, 325. \$34.95.

Vietnamese revolutionary history is often equated with Ho Chi Minh and the Indochinese Communist Party (ICP). Hue-Tam Ho Tai goes beyond this view in her book to explore the broad political-cultural environment of Vietnam under French colonialism. She traces the impulse of radicalism as it emerged from a traditional Confucian society, and she argues that the generation of radicals in the 1920s, seeing an increasing need to struggle against French colonialism, concentrated on issues of individual freedom, especially within the family. According to Tai, however, the generation of the 1930s saw the focus of struggle change from the family to society, as epitomized by Marxist theory. The cultural tension between the individual and the family was replaced by demands for social justice and tensions between the classes. By the end of the 1920s, most radical groups and emerging Marxist organizations had been heavily repressed; Tai maintains that Marxism allowed for a spirit of hope by its deterministic view of victory. Although the Leninist pattern of organization allowed for structure and discipline, Tai stresses that the key point for Vietnamese history was not class struggle, but reclaiming the sense of piety toward the past that allowed the Viet Minh (controlled by the ICP) to effectively mobilize the masses.

Tai's work is a mixture of riveting evidence and succinctly drawn inferences about the very complex radical milieu in Vietnam during the 1920s and 1930s. The sources are diverse, ranging from archival evidence to personal histories. Tai surveys a wide range of radical contenders and themes, including journalism as a transitional mode of activism, feminism and politics, the accommodationists, the expatriate experience, the growing generational alienation that led to increasing student rebellion and political activism, and the difference between the elite-oriented mode of traditional leadership contrasted with the growth of Marxist discourse and Leninist organization. At the heart of the book is the character of Nguyen An Ninh, who, as journalist, activist, expatriate, political organizer, even secret-society figure, epitomized the search for moral culture under French rule.

Tai not only draws the outline, but she fills in the picture with vivid detail and insightful portraiture. There are two difficulties of which the general reader might be aware. The first is the lack of a bibliography. Second, due to the large number of different personalities and groups covered, a short appendix would have been a convenience. These are not overwhelming shortcomings, however. The book will be a valuable companion to previous historical studies by Tai, David Marr, and William Duiker, and will particularly complement the work of Huynh Kim Khanh. Because of its breadth and richly textured material,

Tai's book is a worthwhile resource for the nonspecialist as well as the specialist.

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JUDITH A. STOWE. *Siam Becomes Thailand: A Story of Intrigue*. Honolulu: University of Hawaii Press. 1991. Pp. xii, 394. Cloth \$39.00, paper \$16.95.

Judith A. Stowe begins her book with a discussion of the events leading to the coup of 1932 that terminated the power of the monarchy and ends it with the conclusion of World War II in 1945. During this period, civilian and military leaders struggled for control over a new semi-constitutional form of government against a backdrop of Japanese infiltration and occupation. The author hopes to clarify the events of the period by narrating the activities of the leading political and military figures in chronological order: a story of intrigue. Substance is defined by action, what people were doing and when. Little background is given. Discussion of character and personality are limited. Economic, social, and cultural factors receive minimal attention. The book reads like a transcript of continuous radio or television news reports: meetings were held; proclamations were issued; military actions occurred.

This concept of chronology as history raises questions. Chronology is not necessarily objective. There are always the issues raised by the selection of events mentioned (in this case, the emphasis on military leaders and their activities) and by the attributions and adjectives used to spice up the text. Characters, not always identified, appear and disappear. Identifications, when given, cover only family background, education, and position in government—the same minimal information that appears in official directories. No distinctions are made between major events and minor ones. And there are far too many references to rumors without any effort to find out if they had any basis in reality or if they made any contribution to the sequence of events.

This is a book that confuses more than it enlightens. Much of the intrigue adds up to very little. The Allies won World War II, Japan lost. Neither pro-Japanese nor pro-Allied intrigues in Bangkok had much impact on the final outcome. Even the terms of the peace settlement had more to do with the rivalry between the United States and Great Britain than with the wartime activities of the various Thai factions.

All we end up with here is a paradox. Pibul, the military strongman, appears as a timid, indecisive figure, frequently absent when a crisis occurs; Pridi, the civilian political theorist, comes across as a strong administrator and a capable minister, first in the Department of the Interior, then in the Department of Finance. Pridi's service as regent representing the



royal family and his activities in the Free Thai Movement should have strengthened his political position. Yet after the country returns to normal, Pibul comes out on top, and Pridi loses (again). Why? Chronology alone cannot explain or even clarify the issues that make the 1930s and 1940s such a critical turning point in modern Thai history.

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SYED NESAR AHMAD. *Origins of Muslim Consciousness In India: A World System Perspective*. Foreword by IMMANUEL WALLERSTEIN. (Contributions to the Study of World History, number 29.) New York: Greenwood. 1991. Pp. xv, 311. \$47.95.

Syed Nesar Ahmad's work is one of synthesis and analysis, reviewing a wide range of secondary works to explain "Muslim consciousness," conceptualized as a separate Muslim political consciousness. The book proceeds chronologically, with chapters on nineteenth-century Islamic revival movements in response to British rule; the rise of modernism in the context of the "great depression" of the late nineteenth century; the rise and decline of Hindu-Muslim unity through the course of World War I and into the 1920s; and, in a long final chapter, Muslim separatism during the Depression of the 1930s and World War II. Ahmad's approach has two valuable characteristics: first, to show identities as historically constituted in interaction with social, economic, and political contexts; and second, to show the critical importance of placing those contexts in a larger geographical setting than the boundaries of the nation-states that so often define our histories.

The motor to political action and indeed Muslim consciousness in Ahmad's analysis is elite material interests. Recalling studies by scholars like Paul Brass (for example, his *Language, Religion and Politics in North India* [1974]), Ahmad emphasizes competition among elites who then, as political grids change, deploy cultural symbols in order to mobilize the popular support needed to participate in those grids. Ahmad links class developments to broad patterns of economic change to show that, far from any single communal interest, it is the interests of powerful groups, interests potentially at odds with those of co-religionists, that are at stake. Thus, the *jotedars* of Bengal emerge as a powerful class of small landlords whose interests diverge from the powerful, largely Hindu, big landowners as well as from the Muslim lower peasantry who are, ultimately, persuaded to support them nonetheless on grounds of shared religion.

While suggestive of significant developments, Ahmad's analysis will seem to many readers too singular, particularly because of his neglect of the wholly new context of a public culture, created by new modalities

of communication, that transforms identities and public action. As Immanuel Wallerstein notes in his foreword (p. ix), "If one composed two lists of worldwide names of . . . 'groups,' one list say as of 1500 and one say as of 1950, some on the two lists . . . would be the same nominally. But would they be the same existentially, or sociologically?" Ahmad offers an implicit description of the contrast signaled in Wallerstein's statement, but one would wish for a richer sense of the transition. For that, the approaches of social and cultural history would come into play as they do not here and would be seen as integral to, not separate from, the economic and political structures emphasized throughout. "Hindu" and "Muslim" solidarities—let alone those denoted "fundamentalist," "orthodox," and "heterodox"—are often treated here as historically continuous categories and not as groupings that are in the process of construction.

The topic of this book, the creation of politicized ethnicity and community in the twentieth century, could not be more timely given today's world events. Ahmad was a young scholar tragically killed in an airplane hijacking in 1986. In this posthumously published work, he has raised important questions, and, in his emphasis on economic differentiation and competition, he has left us a significant legacy for further research.

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DOUGLAS E. HAYNES. *Rhetoric and Ritual in Colonial India: The Shaping of a Public Culture in Surat City, 1852–1928*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xi, 363. \$49.95.

Douglas E. Haynes has written a thoughtful book about ideology and nationalism in colonial India. He starts with the founding of Surat municipality in western India in 1852. His interest initially was political history; that this history was written in language familiar to him struck him as strange. He conceptualizes his task as explaining the development of political ideology along liberal democratic lines, looking at symbolic behavior—rhetoric and ritual—to show that Surat's public culture, while constrained by its development under colonial rule, was formulated by the elite through struggle and interaction with colonial officials and institutions.

Haynes sees himself as an ethnohistorian, interested in the construction of cultural meaning, and he puzzles over why the leaders of India's independence movement used the language of liberal, representative democracy when that language ill-served the interests of the "underclasses." In his conclusion, his concern with contemporary India is clear. He suggests that "the most important test of democracies that have grown out of colonial contexts rather than out of demands from deeper within society is whether

liberal political idioms will ever be able to articulate the interests of the underprivileged peoples" (p. 295). Will the underclasses, he asks, always be dependent on elite spokespersons and unable to really shape the political system?

The book is difficult to evaluate, since the argument is continually cast in terms of language, discourse, idioms, and so forth, but most of the rhetoric in the book comes from Haynes. He talks about the "public idiom and the idiom of community" (p. 106), the "idiom of empire" (p. 136), the "partially shared idiom" of Surti Hindus (p. 72), and the "bilingualism" of the elites (merchants, religious headmen, and Mughal notables, and, later, the English-educated politicians) who addressed both the alien rulers and the subordinate groups in Surat city. But we have no extended texts for analysis, no convincing comparisons of indigenous community discourse with public empire discourse. We also have only one (unconvincing) comparison between the language used to address earlier Mughal or East India Company officials and the British imperial rulers (pp. 92–93). But Haynes claims that the English-educated elite employed a political idiom distinct from that of the civic arena to reach the larger body of city dwellers, although "the precise contours of this second idiom are difficult to discern since it was so rarely recorded in documents" (p. 170). Haynes uses primarily English sources, but if his methodology were really grounded in linguistic anthropology or even ethnohistory, the need for textual analysis to strengthen his comparative arguments is obvious. There is no real evidence of how language reflected, distorted, or mystified the power relations at any one time.

A second problem arises when one tries to determine the social groupings implicit in his sociolinguistic arguments. Haynes lays out the actors over time, but his descriptions are curiously self-contained and it is difficult to see groups and actors moving through the book. Furthermore, he never adequately defines the "underclass." Early on, he describes the inner politics of the city and presents the religious and caste groups in the city, their residential and occupational patterns, the value placed on religious giving, and so forth. We learn about Jains and Hindus (both Brahman/Vaniya and Gancha-Goli clusters), Parsis, and Muslims ("immigrant" Mughal elites and local converts, the Patani and Daudi Bohras). Haynes argues that in Surat there was a continuity of the social order, that capitalism, colonialism, and the British legal order did not produce a transformation fundamental enough to form new classes and new forms of discourse. Rather, the notables and later the English-educated elites learned the colonial discourse and used it to accomplish their own ends, reinterpreting key words as they did so but nonetheless reinforcing colonial hegemony and their own subordination.

Worse still, by appropriating the colonial/alien discourse, the elites served as symbolic specialists to the

larger population or the underclasses, who were unable to capture "the nuances of expression key to participation in the civic arena" and "must have regarded civic politics as an esoteric cult—one to whose sacred incantations they were not privy." Thus, the "real underclasses" of the city (whom Haynes promises to define "in a moment" [p. 13]), who are also referred to as "more city dwellers" (p. 187) or "the population" (p. 197) or "subaltern groups" (p. 238), found the developing public culture inaccessible and unchallengeable. (When he does define the underclass as "women, laborers, artisans, and petty traders," the definition raises as many questions about the category as it answers [p. 260].) Only toward the end of the book, when the narrative takes up Gandhian politics after World War I, do we get a coherent sense of movement. The Gandhians did create a language rooted in Gujarati principles of morality that had strong appeal in Surat from 1919 to 1924. Haynes sees this as an underclass politics informed by precolonial idioms confronting the elite politics of constitutionalism. He credits one man, Mahatma Gandhi, with this development, and he locates the development outside of Surat (and India [pp. 203–04]). Haynes seems somewhat ambivalent here, for although this is a strong part of the book, the Gandhian rhetoric achieved "the decolonization of the language of Surti politics" by linking politics and religion in one discourse. The Gandhian anticolonialism "conspired with hegemonic processes associated with colonialism to produce two distinct religious consciousnesses, one Hindu, one Muslim" (p. 262). By 1925, the elites and their liberal democratic discourse were restored to power in Surat, but communalism had become a part of nationalist politics.

Haynes concludes that the "Anglo-Indian political order privileged and reinforced rhetorical efforts to develop appeals built around religious solidarity while discouraging attempts to create alternative languages that could challenge the assumptions of colonialism," thus "creating the illusion that local society had always been divided sharply along the lines of religion." And although he says that most Surtis became participants in public culture through religious collectivities, he also says that the urban underclasses had no voice in shaping the political world that would succeed British rule (pp. 282–83).

One can take issue with some of the arguments advanced in the conclusion and elsewhere, and there are a few minor problems with the production. But much work has gone into this book, and readers will appreciate the stimulating ways in which Haynes has turned to current political issues and social theory to make the material meaningful.

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LLOYD ROBSON. *A History of Tasmania*. Volume 2, *Colony and the State from 1856 to the 1980s*. New York: Oxford University Press. 1991. Pp. x, 663. \$98.00.

This is the second and final volume of Lloyd Robson's *History of Tasmania*. Like volume 1, this book is literally stuffed with facts (mined mostly from primary sources). The facts are all interesting, but Robson often fails to give the reasons for the events and their significance. He delivered the manuscript a few days before he died. To his credit, he leaves an amazingly deep well of information.

Sometimes Robson becomes enthusiastically engaged with a person or an event: for example, Joe Lyons (1879–1939), whose career in the Tasmanian parliament and in the federal parliament, where he was prime minister from 1931 to 1939, was impressive. Another example is the fate of the fourteen Tasmanian aborigines living in 1854, whose sad demise fills the six-page prologue. The author is sharp when stating his own opinions, and his chapter titles are often perceptive; the chapter on World War I is "The Great Madness," and the chapter on the Boer War is "A Yelp At The British Flag."

Among the many interesting facts Robson uncovers is that 852 Tasmanian soldiers fought in the Boer War and two won the Victoria Cross, a rare honor. A few more interesting facts follow seriatim, remembering that Tasmania's history is something of a microcosm of the rest of Australia's history. The slow building of railroads is interesting; but there is no explanation why three feet six inches was the chosen gauge. Unlike on the mainland, anti-Catholicism was not a powerful factor in Tasmanian society. In the 1870s Tasmania suffered through infestations of tiger cats, codling moths, and rabbits, and disastrous bush fires. There was an enormous expression of loyalty for Queen Victoria from 1860 to her death in 1901. In 1875, seventeen family groups owned forty-five of the largest one hundred properties. Before 1900 few Chinese immigrated. Mostly market gardeners, they were never welcome on the mining fields. After 1890, the trade unions were unpopular with the government, especially those of the sheep shearers and the dockers. When the century turned, education and health care still remained poor. In contrast, silver and copper mining boomed at Mount Lyell.

From the 1880s until today the tourist trade flourished. Close to Hobart, capital of Tasmania, is Port Arthur, early convict settlement and cruel reminder of the penal system. Sport is given a good account, especially cricket. Robson mentions the great fast bowler E. A. McDonald, who was born in Launceston. His approach to the wicket impressed onlookers for "its effortless, gliding motion, termed by Neville Cardus 'a silent run of sinister grace'" (p. 304).

Tasmania did its fair share in world wars I and II, although the two conscription referenda of 1916 and 1917 were voted down. The author has little to say

about "The Dismal Decade" (1925–35); but he describes the postwar period within the framework of "The Greening of Tasmania."

The book is a valuable resource and reference for studying Tasmania. The bibliography is superb, and the book is handsomely printed and illustrated—a credit to the Oxford University Press, although their price is excessive.

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NICHOLAS THOMAS. *Entangled Objects: Exchange, Material Culture, and Colonialism in the Pacific*. Cambridge: Harvard University Press. 1991. Pp. xiii, 259. Cloth \$32.50, paper \$14.95.

Nicholas Thomas presents a historically grounded critique of one-sided approaches to exchange in Oceanic societies. On the one hand, some anthropologists have proposed that these island societies are primordial "gift economies," in extreme contrast to commodity-based economic systems; in gift economies, presentations are "inalienable" in continuing to embody some essential characteristic of the donor and transactions create enduring social relations of indebtedness. On the other hand, some have assumed that the differential economic effect of the colonial encounter can be attributed entirely to variations in the penetration of global capitalism. Thomas takes a sensible perspective that answers both of these extreme positions: Oceanic societies need to be seen as having simultaneously both gift and commodity forms (roughly, objects indexically linked to exchange participants and objects linked by a conversion calculus), each operating in different spheres with different material objects as the embodiment of value. Grasping this range of local complexity is a precondition for understanding contact situations.

In his treatment of specific instances of encounter between islanders and colonial agents, Thomas recognizes the devastating, often fatal consequences of contact without losing sight of the importance of local assumptions about the meaning of objects and the patterns of exchange. In the early nineteenth century, for example, Fijian chiefs eagerly participated in economic exchange with foreign traders, whose desire for *bêche-de-mer*, sandalwood, tortoiseshell, and pigs did not directly impinge on the already elaborated ceremonial exchange system based on whale-tooth valuables. In the Marquesas, by contrast, competitive pig feasting was a significant index of social inequality, so traders' demand for pigs had the potential to seriously undermine local political activities. As Thomas summarizes: "The circulation of objects, especially across the edges of societies, civilizations, and trading regimes, is not merely a physical process but is also a movement and displacement of competing conceptions of things, a jostle of transaction

forms. Global economies do not control the meaning of the commodities that their profits turn upon, even if the appropriation of these goods in the form of gifts, commodities, or prestige valuables inevitably entangles receivers in wider relations that are not easily shrugged off" (pp. 123–24).

In line with this bidirectional approach to colonial encounters, Thomas presents a selective history of the European appropriation of objects from Oceanic societies. The focus here is not on obvious things such as raw materials, prestige goods, and human laborers that dominated the economic picture for several centuries, but rather on the collection of native "curiosities" by voyagers, government agents, missionaries, travelers, and scientists. Thomas senses a transition around 1800 in European attitudes, from the unstructured, nonevaluative collecting of striking novelties to more systematic collecting aimed at demonstrating the links between material artifacts, evolutionary stages, and the cultural differentiation of island societies. Crossing both sides of this approximate boundary, an additional function of artifacts is marking the unique, personal experience of the traveler whose cross-cultural encounter is mediated and reified in the trophies brought home.

The final chapter brings "a limitless succession of cultural reconstructions of the identity of things" (p. 187) up to date with a discussion of the parodic imitation and oppositional appropriation of European goods. In neotraditional Fijian villages, for example, the distinction between Fijian and foreign custom is expressed by the opposition between gifts and commodities. Although this clearly argued book deals almost exclusively with Oceanic contexts (with a focus on Fiji), historians concerned with the broader issues of colonialism and with the historicity of material objects will find much of interest in it.

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#### UNITED STATES

ANNE FIROR SCOTT. *Natural Allies: Women's Associations in American History*. (Women in American History.) Urbana: University of Illinois Press. 1991. Pp. xii, 242. \$29.95

In this study of women's associations in the United States from the revolution through the 1920s, Ann Firor Scott has admirably synthesized existing scholarship and broken some new scholarly ground to create a broad overview that should remain a standard source for some time to come. Beginning with the female benevolent societies of the 1790s, Scott charts the development of the myriad of women's religious, benevolent, and reform organizations that have come into existence throughout American history. She follows the standard typology: in the antebellum era benevolent societies gave way to reform associations, which then gave way to the Civil War

"soldiers aid" societies. For the post-Civil War era, Scott confines her attention to religious, self-improvement, and community improvement associations. All of these organizations, she contends, made the attainment of woman suffrage inevitable and contributed substantially to the Progressive Era social justice movement. Taken together, women's associations, according to Scott, form a significant chapter in American history that general historians have undervalued, for these associations have lain "at the very heart of American social and political development" (p. 2).

Scott's developmental typology is not new, and other familiar themes echo in this work. These include the nineteenth-century notion of female moral superiority and the early twentieth-century notion of municipal housekeeping. Both of these ideas, based on women's familiar experience, emboldened women to leave their homes for voluntary activity. Scott reiterates previous arguments by charting the development of a feminist consciousness in the context of women's empowering organizational work in their voluntary societies, and by linking women's extensive charitable activity during the Civil War to the burst of women's organizational activity during the half century or so after that war.

New and challenging interpretations nevertheless abound in Scott's work. In contrast to scholars who have viewed many of these organizations as elitist and bent on social control, Scott stresses their positive contributions in creating vital community institutions, in structuring cross-class alliances, and in challenging the dominant ideology of unregulated capitalism by offering an alternative vision of a just and humane society. She suggests that women's organizations preceded those of men and that women's greater connection with home and family afforded them a more immediate empathy with the disadvantaged. Women's organizations provided an "early warning system" important to capturing the attention of the male-dominated political process for reform initiatives (p. 3). In addition, Scott carefully maps the development of black women's organizations, noting that their antislavery and educational societies preceded those of white women.

Contrary to recent arguments by historians such as Joan Scott and Linda Kerber, who have called for a reorientation of the field of women's history around the concept of "gender" rather than of "women," Scott amply demonstrates the value of the continued investigation of women's separate experience of reality. Moreover, her analysis of what she calls "organized womanhood" points to the actual existence of a separate sphere of action for women, which Carroll Smith-Rosenberg posited some time ago and which has been recently contested. Among Scott's innovative hypotheses are that women entered different reform activities from men, that experience in women's separate organizations influenced the consciousness of women professionals to create a mentality



among them different from that of male professionals, and that women's organizations through existence and ideology played a role in shaping the developing nineteenth-century middle-class self-definition. These lines of analysis provide rich possibilities for new scholarship and interpretation.

Scott has written an exemplary study that combines existing interpretations with new ones and whose broad scope covering nearly two centuries is admirable. By and large she ends her analysis in the 1920s; one only wishes that Scott had more fully turned her interpretive skills to more recent eras.

LOIS W. BANNER

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HERBERT APTHEKER. *Anti-Racism in U.S. History: The First Two Hundred Years*. (Contributions in American History, number 143.) Westport, Conn.: Greenwood. 1992. Pp. xvi, 246. \$47.95.

Herbert Aptheker has written other books that have altered prevailing views about important matters concerning the history of the United States. For example, he has dealt with the American Revolution, the abolition movement, and the efforts of African Americans to overthrow slavery. His *American Negro Slave Revolts* (1943) helped bring about a major and salutary change in the treatment of this nation's history. That now-famous book appeared a half-century ago.

Now Aptheker offers another readjustment of our intellectual and moral sights. This present volume on the first two hundred years of "anti-racism" in the United States ends with the American Civil War, but another volume is promised, which will take the story into the early twentieth century.

The author deals almost exclusively with "anti-racism" among white people in the United States as it has focused on African Americans. He contends that anti-racist views have been slighted by historians and frankly asserts that this book is a "revisionist effort" to rectify this erroneous neglect. He cautions that "In rectifying errors, one must be careful to avoid exaggeration" (p. xiii).

He has compiled an impressive catalogue of the many denunciations of racial prejudice that were made in the United States before the Civil War and has added instances from English and French sources. The book is organized in a way that is strikingly similar to *American Negro Slave Revolts*. After reviewing various historians' treatments of the matter, topical matters are presented—such as "Literature," "Sexual Relations," and "Joint Struggle"—and then there is a shift to marshaling the data in chronological order. One result is a good deal of acknowledged repetition. But without minimizing the magnitude of racist beliefs, Aptheker makes a strong case for the existence of a powerful "anti-racist" tradition.

In doing so, however, he has not avoided exaggerations. Surely it stretches the case to claim "anti-

racism" in descriptions of slave runaways as "very sensible and artful," "can read very well," and being able to play the fiddle "exceedingly well" (p. 48). It requires a particular commitment to see no racism in an antislavery statement that "The human race, however varied in Colour or Intellect, are all justly entitled to Liberty" (p. 92). For the most part the author maintains his customary faithfulness to original sources, although in attempting to stress the early emergence of anti-racism he distorts Samuel Sewall's thinking by quoting (p. 75) only part of a sentence about blacks that in fact ends with a reference to "extravasate Blood."

The author's presupposition that racism was the ideological rationalization of modern slavery results in "anti-racism" having a certain timelessness. He insists that "the data . . . demonstrate very widespread questioning among white people in all sections of the nation and in all periods of its history of the myth of 'Negro inferiority'" (p. 2). I remain unconvinced that there was no appreciable change in such opinions during the revolutionary era, or indeed in other periods and in different places. A structural approach to the problem necessarily finds more constancy and persistence through time and place than one more culturally oriented. Nonetheless, this book presents a great deal of evidence that shows racism met considerable opposition in this country for many years. Whether that opposition was successful is another matter.

WINTHROP D. JORDAN

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THOMAS R. COLE. *The Journey of Life: A Cultural History of Aging in America*. New York: Cambridge University Press. 1992. Pp. xxxv, 260. \$27.95.

Thomas R. Cole makes a fine contribution to the growing literature on the history of aging in America. Unlike other works, his "is not so much a history of attitudes toward old age as it is a study of the historical meaning of aging" (p. xvii). Cole argues that, as America developed from its Puritan beginnings, it moved farther and farther away from an emphasis on the spiritual dimension of aging toward a dependence on science, medicine, and technology to manage and mitigate old age. In the process, aging became increasingly impoverished in its cultural and symbolic dimensions.

Cole divides the history of the meaning of aging into three periods: colonial, Victorian, and modern scientific. Puritan America viewed aging as both a positive and negative experience. The trials of aging offered the elderly opportunities to develop spiritually right up to the moment of death. The rise of the market economy, the spread of Enlightenment ideas, and the spread of evangelicalism and republicanism, however, all worked to undermine the age veneration



of the colonial era and to diminish the spiritual importance of aging.

Cole sees the Victorian era as crucial to the transformation of aging from meaningful to meaningless. His analysis differs in some respects from Andrew Achenbaum's focus on Romanticism as crucial to the denigration of old age (*Old Age in the New Land* [1978]). In Cole's view, Protestantism's emphasis on the perfectibility of humans and the individual's responsibility for moral and physical self-control contributed to society's increased stress on physical health and economic self-reliance. Popular health reform also hurt the elderly by placing blame for physical deterioration on the individual. Gradually there arose an ideal of life that called for a long, healthy life span followed by a painless death. The ailing and dependent elderly violated this ideal. This Victorian image of old age was thus dualistic: there was good, healthy old age and bad, dependent, and sickly old age.

The third period of meaning for aging was ushered in by late-nineteenth-century scientific investigations that produced a negative picture of old age. In the twentieth century, aging has become a problem to be solved by medical, social scientific, and technical means. The rise of professional care for the aged has led to a focus on adjustment as the most significant problem of aging. Efforts since the 1960s to debunk the myths and negative stereotypes about aging, Cole argues, will not solve the essential problem. Society continues the Victorian dichotomy of a good old age of self-reliance and health versus a bad old age of illness and dependency.

Cole specifically limits his study to white, largely urban, middle-class culture. His outline of changing perceptions is convincing and fits with what we know of the history of old age. Whether the historical trends in the meaning of aging that he uncovers are the same for Catholic, Jewish, and minority populations, however, is a question awaiting further investigation.

This brief review cannot do justice to Cole's thoughtful discussion of such subjects as *Pilgrims' Progress* (1728), artistic renderings of the stages of life (from medieval woodcuts to Jasper Johns), or G. Stanley Hall. He firmly ties these cultural artifacts and historical actors to the shifting meaning of aging. But the particular connections between the influential forces—such as the rise of the market economy or modern dependence on scientific expertise—and shifts in the cultural meaning of aging are not as firmly tied together. Without close examination of diaries or some other historically preserved window on the psyche, the exact nature of the influence of these forces on individual experiences of aging will remain obscure. This is not, however, the project Cole has undertaken. His book provides balance to the historical literature of aging that has paid more

attention to the "problems of old age" than to the "meaning of aging."

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EDWARD LAZARUS. *Black Hills/White Justice: The Sioux Nation versus the United States, 1775 to the Present*. New York: HarperCollins. 1991. Pp. xvii, 486. \$27.50.

This book is an uneven chronological recounting of the long struggle to resolve the Sioux Nation claims for the loss of the Black Hills of South Dakota in 1877. Edward Lazarus has a great advantage in researching this topic since his father, Arthur Lazarus, was one of the attorneys handling this case when it was finally decided. This intimacy has both strengths and weaknesses. Lazarus knows the cast of characters that spent the major part of this century working to secure compensation for the Sioux, but he also is obviously devoted to providing an apologia for his father's activities. Thus, this book is not history in the traditional sense of the outside scholar or observer evaluating a record and reaching conclusions. The conclusions came with the territory.

In spite of the advantages of first-hand contact with many of the principals, some jarring sentences indicate that Lazarus may not have even visited South Dakota when writing this book: the geography is frequently askew and sometimes the people and events seem to be dislocated. More astounding, one of the pictures allegedly showing conditions on the Sioux reservations during the Depression is actually a file photo of a Navajo mother somewhere in the Southwest, probably around Window Rock, Arizona. If there is no recognition by editor or author that this woman is a Navajo, how comfortable can we be that any of what is related to us is accurate?

This study is the first book-length effort to describe a prolonged and complicated controversy and so we can applaud Lazarus for laying out a basic chronology, complete with the actors, so that scholars in the future can work within some identifiable set of circumstances. Indeed, the basic chronology is the most important part of the book since the twists and turns of the Black Hills case are so complicated that few people, even Native Americans and scholars familiar with the subject, can work their way through the materials with any degree of confidence.

From a historian's perspective, however, the interpretation that Lazarus attaches to these events is somewhat naïve. When Ralph Case originally took the Black Hills case, federal Indian law was in its infancy. Everything was done on an ad hoc basis and no one—in the government bureaucracy or in Congress—had any idea how a case of this magnitude should be handled. Previous efforts by tribes to gain a measure of justice in the Court of Claims had been frustrated by jurisdictional acts that enabled the

Court of Claims to turn aside Indian efforts on linguistic technicalities. To judge Ralph Case's efforts by modern conceptions of law that had been developed rather significantly by the time that Lazarus and Marvin Sonosky became the tribe's attorneys therefore is to paint Case unfairly as an incompetent, for he certainly was a capable attorney.

By the time the case was completed, federal Indian law had changed substantially. The Taos Pueblo had excluded part of its sacred lands—the Blue Lake area—and received land and money so that the Sioux traditional people had a legitimate complaint that some effort to receive lands and dollars should have been made. The question also arose, as the case came to an end, whether or not the Sioux were actual parties to the complaint because the attorneys, against the wishes of their clients, sought termination of the case.

Finally, there is and always will be an underlying question about the quality of representation offered by Sonosky and Lazarus to the Sioux people. A significant number of points, historical and legal, seem to have been settled by stipulation. Attorneys can explain their compromises as bona fide efforts to cut through red tape and defend their clients' best issue and interests. It is another thing, however, to surrender some forms of claims in an effort to move the major issue through the courts. Since these claims cases were supposed to be the definitive settlement of outstanding claims against the United States, it seems hazardous at best to have stipulated away some possible claims, because the Sioux people have believed and continue to believe that everything should have been settled in a straightforward manner without giving up any tracts of land that the tribe did not willingly surrender a century ago.

It is comforting to remember that Lazarus does not claim to be a legal historian or in fact a mainstream historian, and his primary interest is to resolve for himself a controversy in which his family played a prominent and historical part. Thus, there is as much catharsis as authoritative interpretation in this work, and we should take note of this orientation. We can, however, thank Lazarus for attempting a major work covering a complicated subject.

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CYPRIAN DAVIS. *The History of Black Catholics in the United States*. New York: Crossroad. 1990. Pp. xvii, 347. \$24.95.

When the Reverend George Stallings of the archdiocese of Washington, D.C., captured the attention of the national media in 1989 with his plans for an Afro-centered Roman Catholic parish, he provided a reminder that Roman Catholicism was a force among North American as well as South American blacks. As

Cyprian Davis tells us in his well-researched study, there were 1.5 million black Catholics in the United States in 1990, settled essentially in southern Louisiana and the major urban centers of New York, Chicago, Washington, Miami, and Los Angeles. A religious minority within a racial minority, black Catholics have suffered from neglect in the past because of their beliefs and their color.

Yet as Davis points out, we miss a subtle but significant ingredient in our secular national history as in our religious history if we overlook the story of black Catholics. Noting that the African presence has been a pervasive influence in Catholic history, Davis observes that "more than this, the Catholic church was as much affected by the issue of slavery and its aftermath as any other American institution. The issue of slavery affected the American episcopate in its own understanding of what freedom, justice, and human dignity really mean; the issue of slavery and the evangelization of the African American affected the relationship between the American episcopate and the Roman Curia." One of the real advantages of Davis's work is that while it is a narrowly focused study of Roman Catholic African Americans, it never loses sight of the larger religious and secular context within which the drama of black Catholics unfolds.

Davis provides a balanced and objective account of the relationship of black Catholics with their church. A Benedictine monk, Davis does not gloss over the failure of the church to act with moral courage. Indeed, he points out that members of the church hierarchy and religious orders owned slaves themselves. The church condoned racially separate parishes and racist policies as the postbellum free black population grew in size. It is in noting the painful and complicated situation in which slaveholding Jesuits found themselves that Davis provides a poignant observation on how personal, parochial issues were often connected to the larger national evil.

Yet he also documents the dedicated individuals within the church who took bold action against injustice, people like the Blessed Katherine Drexel, Pope Gregory XVI, Archbishop John Baptist Purcell of Cincinnati, the paternalistic Father Slattery of the Josephites, Archbishop Janssens of New Orleans, Father William Markoe of St. Louis, and the Jesuit John LaFarge. He also tells of repeated acts of daring and commitment on the part of black Catholics themselves to gain a place within the church.

Few endured more for their faith than the early black women religious, who suffered from privation and lack of recognition. When Archbishop Perche of New Orleans saw a black sister in her new habit he rebuked her with the challenge, "Who do you think you are?" Similarly, the first group of black priests were alternately ignored and defined in terms of prevailing negative stereotypes.

Davis's work preserves the historically significant experiences of these dedicated people while acknowledging that his study is not chronologically balanced:

only the last fifty pages or so treat developments in the twentieth century. He begins his discussion in New Testament Africa and devotes considerable attention to the American colonial era and then the antebellum period. But these are just the eras with which we are likely to be the least familiar, and he justifiably treats them at some length. His research is wide, if not deep, owing to the nature of the sources. He has scoured the archives of local dioceses and religious communities and conducted interviews with some of the modern pioneers and their descendants. The book is written with clarity, historical sensitivity, and objectivity. A must for specialists in African-American religious history, this study is recommended reading for anyone in American social history.

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Hobart and William Smith Colleges

STEPHEN HOWARTH. *To Shining Sea: A History of the United States Navy, 1775–1991*. New York: Random House. 1991. Pp. xv, 620. \$25.00.

In this book, Stephen Howarth recounts the “story of the rise, and fall, and rise again of the U.S. Navy” from 1775 to 1991 (p. xiii). Apparently intended for a general audience, the volume provides a gracefully written and smooth-flowing narrative. Howarth does not break new ground but follows a traditional interpretation of naval history. He usually accepts uncritically Alfred Thayer Mahan’s proposition that nations should maintain large navies that exercise command of the sea.

In general, Howarth’s book is based on readily available secondary works. Missing from his bibliography, however, are many important works on the navy that have appeared over the past generation, including studies such as Peter Karsten’s *Naval Aristocracy* (1972), Robert Seager’s *Alfred Thayer Mahan* (1977), David Long’s *Gold Braid and Foreign Relations* (1988), and the series of biographical essays edited by James Bradford. Although one would not expect exhaustive research in a popular history, the problem is that the bulk of recent scholarship apparently was not examined. This fact limits the usefulness of the book.

Howarth is not entirely unaware of challenges to older interpretations, but he does not feel the need to reconcile or choose between rival approaches. In his narrative of the 1820s and 1830s and again of the post-Civil War navy, Howarth adopts a Mahan-like displeasure with the nation’s lack of interest in developing a large fleet prepared for offensive operations against potential enemies. He then briefly notes the more recent arguments that a smaller navy was actually better suited to the needs and resources of the United States at that time, but he does not attempt to explain these conflicting ideas.

Despite his lack of interest in new interpretations and his reliance on older secondary works, Howarth’s book could serve as a well-written introduction to naval history. Howarth is notably successful in providing accounts of naval engagements. In his description of World War II, for example, he handles the complexity of the two-ocean war with great skill. In the Cold War period, however, Howarth’s account becomes an episodic examination of scattered people and events. This weakness may reflect the fact that there are not yet good general histories of the navy since 1945 on which he could rely.

Another strength of the book stems from Howarth’s ability to draw on his knowledge of the British and Japanese navies to make comparisons among the three services. These comparisons appear both as intriguing asides and as a valuable context for the development of the U.S. Navy. For example, Howarth compares David Porter’s voyage in the Pacific during the War of 1812 to earlier raids by Sir Francis Drake. He also places the development of the navy in the context of the growth of the Japanese force in the twentieth century.

This volume is one of three histories of the U.S. Navy to appear over the past year. Although all span the years since 1775, each takes a unique approach. Kenneth Hagan’s *This People’s Navy* (1991) emphasizes strategic issues and subjects Mahan’s concept of naval history to a prolonged and devastating critique. Robert Love’s *History of the U.S. Navy* (1992) is a two-volume work that develops links between naval and political/diplomatic history in greater detail than the other two. For students with little knowledge of naval history, Howarth’s book may be the best place to start, with the caveats noted.

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RICHARD MAXWELL BROWN. *No Duty to Retreat: Violence and Values in American History and Society*. New York: Oxford University Press. 1991. Pp. 268. \$24.95.

Richard Maxwell Brown has great timing. This book on violence and American values was fresh on the shelves when riots erupted in Los Angeles. Regrettably, timeliness is no substitute for persuasive analysis. Even the work of a master historian is bound to disappoint when it offers assertions instead of evidence and establishes no convincing link between cause and effect.

Brown’s argument is clear enough: English common law required a person under assault to retreat to the wall before killing in self-defense. In America the law gradually changed to reflect a different standard: the right to stand firm when challenged, with no duty to retreat before taking another person’s life in self-

defense. This Americanization of the common law, adopted especially by courts west of the Appalachians, represented widespread social values and ultimately found favor in a decision of the U.S. Supreme Court written by Justice Oliver Wendell Holmes in 1921.

Few students would dispute Brown's contention that aggressiveness is a defining American characteristic. But Brown pushes the argument much further. He sees the legal doctrine of no duty to retreat as the reason why we live in the most violent society in the Western world. It is impossible, he writes, to count the lives lost "as a result of the change in the law . . . but the toll of victims . . . may well be, indirectly but significantly, in the tens of thousands" (pp. 5-6). And the impact of standing one's ground extends far beyond the nation's boundaries, even affecting the conduct of foreign policy.

Brown traces this theme through several chapters, from the evolution of the doctrine in Ohio, Indiana, and Texas to the role of gunfighters in the West to a detailed account of the Mussel Slough shootout in 1880. A final chapter and separate conclusion range more widely, with Bernhard Goetz, youth gangs, the Information Society, "mountain man" Claude Dallas, Wyatt Earp, and President Dwight Eisenhower all receiving attention. The common thread in these examples is Brown's claim that they illustrate the triumph and attendant dark results of the ethic of no retreat.

Historians will find much to fault in this brief book. The legal analysis scarcely extends beyond an unsatisfactory parsing of two state appellate cases, some Texas statute law, and a couple of divergent Supreme Court opinions. There is no look at criminal prosecutions, so Brown cannot know how the doctrine of no retreat played out during trials. His unsupported assertion that a late-nineteenth-century Western Civil War of Incorporation produced "thousands" of hitherto unacknowledged gunfighters (p. 60) will meet dispute from the careful scholars who have identified many fewer such figures. The conclusion that no duty to retreat governed the killings at Mussel Slough does not square with the discovery that the California Supreme Court failed to adopt the doctrine until 1897, seventeen years after the tragedy (p. 218, n. 110). And scholars will challenge Brown's assumption that a change in legal doctrine, rather than Civil War experiences, which Brown scants, led to such a dramatic shift in behavior.

D. H. Lawrence once described the essential American soul as "hard, isolate, stoic, and a killer." Certainly events in our age appear to support this conclusion. But in this work, Brown does not explain convincingly why it is so.

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ALLAN J. LICHTMAN and KEN DECELL. *The Thirteen Keys to the Presidency*. Lanham, Md.: Madison. 1990. Pp. xiii, 449. \$24.95.

During the 1950s political scientists increasingly used voter surveys to analyze electoral behavior. V. O. Key, Jr., in his classic *The Responsible Electorate* (1964), vigorously criticized such works for viewing voters as responding to campaign stimuli rather than as rational citizens evaluating government policy and performance. This book by Allan J. Lichtman and Ken Decell takes a parallel position, dismissing current wisdom about the vital importance of campaign strategies, television, and interest groups, and arguing that presidential elections "are referenda on the broad-based performance of the executive party" (p. 396).

The authors quickly dismiss the value of polls in predicting winners and reject various theories stressing the importance of party identification, electoral patterns, salient issues, centrist ideology, and prosperity. This is only brief preparation, however, for presenting their own model of thirteen factors or "keys" which together predict—and explain—presidential elections. They present these as dichotomous, true-false propositions. The first four factors are political: an increase in incumbent party congressional seats between midterm elections, no serious contest for the incumbent party nomination, an incumbent president, and no significant third party campaign. Seven keys reflect government performance: the economy (short and long-term), foreign/military issues (absence of failure and achievement of success), the absence of serious social unrest or scandal, and whether the administration instituted any major domestic policy changes. The final keys measure the "charisma" of incumbent and challenging party candidates.

Analyzing the predictive value of the keys for each presidential election from 1860 through 1988, the authors conclude that if five or fewer of these propositions are false, the incumbent party will win; if six or more are false, the incumbent party will lose. The bulk of the book (282 pages) is their evaluation of each of the thirty-three presidential elections in terms of the thirteen keys. This provides only six to ten pages per election, but the summaries are judicious and insightful.

What makes this approach particularly interesting is its conclusions about "rationality" and policy orientation of voters. Despite diligent efforts, however, the authors do not fully demonstrate this perspective, nor do they fully explain how or why this model works. What most needs developing is the interaction between the keys. Although they address this for some keys and elections, they do not show general patterns of interaction.

Constructing the model is also problematic. The dichotomous structure of the keys seems unnecessarily blunt. Many keys are well defined, but others seem



vague and subjective. The weakest involves evaluating by unspecified method the electorate's perception of scandal. Of greater concern is that all keys have equal importance in the overall prediction. Finally, and most troublesome, there is no accounting for any of the considerable changes in 138 years: the rising importance of foreign policy, the shifts in party loyalty or methods of nomination, and the changing nature and significance of policy making.

Perhaps none of these problems are insurmountable, and the authors have already grappled with some of them. It is also interesting to have a somewhat optimistic reading of the history of American presidential elections. Despite its thought-provoking value, however, this theory needs further elaboration before it will displace existing electoral explanations.

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LOIS GREEN CARR *et al.* *Robert Cole's World: Agriculture and Society in Early Maryland*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1991. Pp. xxi, 362. Cloth \$39.95, paper \$19.95.

Lois Green Carr, Russell R. Menard, and Lorena S. Walsh have calculated and explicated colonial Maryland's demographic, economic, and social history with empirical precision in dozens of important articles and several monographs. This book is, nevertheless, long awaited. The authors' vividly written story about the flesh-and-blood colonial Marylanders of a 300-acre St. Mary's County tobacco plantation on the banks of St. Clement's Bay a few miles from the Potomac River examines the world of Robert Cole (ca. 1628–ca. 1662), his wife Rebecca (d. 1662), his executor Luke Gardiner (1622–74), and the Coles' children, stepchildren, servants, and hired laborers.

For more than two decades Carr, Menard, and Walsh have been debating evidence, methodology, and conclusions about this society. Their mastery of the vast specialized scholarship enables them to discern both representative and idiosyncratic aspects of Robert Cole's world as it is reflected in two unusually detailed primary sources (printed as appendixes): his will, dated 1662 and proved in September 1663 (pp. 170–74), and Luke Gardiner's account of his guardianship of the Cole estate from 1662 to 1673 during the minority of Robert Cole's eldest son and namesake (pp. 174–207). Other appendixes provide data about the household members, laborers, and livestock at Cole's plantation, and short biographies of persons mentioned in Cole's will and Gardiner's account. Seventy pages of notes and commentary round out the volume, which provides virtually everything needed for a colloquium of bright undergraduates.

Cole's plantation and its Chesapeake neighbors participated in a transatlantic market economy that quickly eroded Lord Baltimore's medieval vision of

manors in the New World. Although early Maryland farms fell short of English pastoral ideals, community institutions and human networks soften the harsh and chaotic "profit maximizer" stereotypes of earlier Chesapeake scholarship. Between Cole's arrival in 1652 and the end of Gardiner's executorship in 1673, Maryland was transformed from a beleaguered outpost of 200 souls into a substantial agricultural colony of 8,000 inhabitants. Older areas of Virginia witnessed their analogous transformations earlier, but on both sides of the Potomac the mid-seventeenth century was the "Age of the Yeoman Planter" (p. 17). The composition of the Chesapeake population soon changed with the monumental transition to slave labor that accelerated after 1680, but, regardless of whose hand held the hoe or the plow, the area remained a staple-crop agricultural society. Colonial days and seasons answered the demands of the family farm: even "slavery did not change work routines for the great majority of whites. Most men and boys remained in the fields; women and girls still worked at or supervised household tasks" (p. 163).

In this splendid reconstruction of the agricultural world of a representative early Chesapeake yeoman family, Carr, Menard, and Walsh introduce us to ancestors both of Charles Sydnor's gentlemen freeholders and of Rhys Isaac's eighteenth-century planters and farmers.

JON KUKLA  
Historic New Orleans Collection

PETER C. MANCALL. *Valley of Opportunity: Economic Culture along the Upper Susquehanna, 1700–1800*. Ithaca: Cornell University Press. 1991. Pp. xviii, 253. \$29.95.

Students of eighteenth-century Pennsylvania know a fair bit about the southeastern corner of the commonwealth. Philadelphia is well served, and Chester County is not far behind. Peter C. Mancall turns our attention to the Upper Susquehanna Valley, which takes in much of northeastern Pennsylvania as well as parts of New York State, in this useful work. He brings together and expands on material that had been scattered in various sources. But the book breaks little new ground, and the one "thesis" that occasionally emerges can be questioned.

Mancall's approach is a comprehensive one. He examines the physical environment—the climate, soils, crops, and animals of the valley. He discusses methods of farming and looks at the evolution of the regional transportation network.

Several chapters focus on the native peoples, and here the story is one of decline. After the Susquehannocks left the valley in the seventeenth century, other tribes moved in. Among these were Conoys, Tutelos, Tuscaroras, Shawnees, and Delawares. But supplies of beaver dwindled and the pressure from European settlers increased. The Treaty of Fort Stanwix (1768)



marked the end of substantial native settlement in the valley. The American Revolution was another blow to Indians in northeastern Pennsylvania and western New York. Most sided with the British, and most suffered terribly when American forces destroyed their homes and crops.

White settlement came in spurts, with the strongest waves of growth coming between 1768 and 1776, and after 1785. This migration was marked by tension between Europeans and natives, Pennsylvania and Connecticut settlers, and landlords and tenants. Much of the land was originally taken up by speculators and wealthy investors like Robert Morris, George Croghan, and William Cooper (father of the novelist, James Fenimore Cooper). Landlords struggled to sell or lease their land to small farmers. Mancall might usefully have contrasted his findings to patterns in Maine, western Massachusetts, New York, or other parts of Pennsylvania. Large holdings and tenantry were common on the early American frontier and the key questions become comparative ones.

In the opening and closing chapters, and occasionally in the text, Mancall discusses the "capitalist transformation" of the Upper Susquehanna Valley. And it is here that his argument seems most questionable. "At the beginning of the eighteenth century," he writes, "economic ideals reflected a strong sense of community." But by 1800 "the economic world inhabited by these peoples was decidedly market-oriented" (pp. xii–xiii). Mancall does not grapple with the careful reasoning of historians such as James Henretta, Michael Merrill, or Christopher Clark, who argue that the transition to capitalism in the rural North came only in the 1830s or 1840s. Mancall shows that farmers in the valley exported some of their crops. But that hardly makes them capitalists. More to the point is the relative importance of production for local exchange and production for distant markets. And even in 1800, it seems, the typical valley farmer tended a variety of crops and animals, rather than concentrating on a few products for market.

This work provides valuable information about the Upper Susquehanna. Several maps show the location of towns established by Indians and colonists. An appendix presents data on population and landholdings. But this is also a book that would have profited from a more incisive approach. There are lost opportunities in this study of the Susquehanna Valley.

MARC EGNAL  
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JOHN FREDERICK MARTIN. *Profits in the Wilderness: Entrepreneurship and the Founding of New England Towns in the Seventeenth Century*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1991. Pp. xiv, 363. Cloth \$34.95, paper \$12.95.

This book is limited in scope but contains perceptive insights into the history of seventeenth-century New England. John Frederick Martin stays tightly focused on his announced subject of entrepreneurship and the founding of towns. He acknowledges that "a single town had many facets—as home to a church, a military outpost, a community of neighbors"—but that he plans to study only the business enterprise "side to town founding" because historians have paid so little attention to the subject (p. 5). His chapter titles include "The Leading Entrepreneurs," "The Creation of Land Corporations in Towns," and "The Use of Shares." Puritanism appears often in the index, but not as often as terms like "corporations" and "land companies." The bibliography contains little on theology, birth rates, and family size, but much on English corporate history and land law.

Martin's findings, based on a study of sixty-three or about half the New England towns founded before 1700, provide ample evidence that profits were fully as important on the frontier as was saving grace. "Towns," Martin argues, "apportioned rights and responsibilities according to business principles" (p. 3). Entrepreneurs invested in communal land corporations, acquiring "rights" or "shares" that could be sold, developed, leased, or held for capital gains. The land corporations became powerful communal institutions. Resident shareholders, known as "inhabitants," constituted a landholding elite. For most of the seventeenth century the corporations shared local authority with church and town.

The most original part of Martin's story is his explanation of how the "ambiguous" relationship between town and corporation was resolved. Lack of clarity about which group was responsible for financing community affairs and who should share in land distributions caused a series of conflicts throughout New England. Hardly a town remained unaffected. Then, during the Dominion, Governor Edmund Andros announced that towns had no legal existence according to English law. The land corporations did, but not the towns. After the Dominion collapsed, colony after colony passed laws clarifying the relationship between the two. The laws all distinguished clearly between a corporation of town proprietors who controlled land and a corporation of town residents who controlled governance. Thus, the unwilling architect of policies that shaped most subsequent town founding in New England was none other than the region's most threatening seventeenth-century official, the infamous Andros.

In addition to straightening out a tangled sequence of economic developments, Martin successfully relates these developments to some of the broader historiographical questions of concern to colonial New England historians. For the most part his contribution involves bringing good sense to bear on the either/or nature of debate. In a brief chapter entitled "Commerce and Culture" he notes simply that entrepreneurship and piety went hand in hand, in part

because Puritanism involved a commitment to occupying the wilderness. Martin also discusses communal idealism, relating economic covenants to various forms of religious and social covenants. He concludes that "These various covenants not only bound people to each other but also roped them off from others; indeed, that was a main purpose of them . . . Puritans were in the habit of sorting people into their own inviolable compartments" (p. 236).

With Martin's book, we have a much fuller understanding of the entrepreneurial compartments of seventeenth-century New England.

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LELAND FERGUSON. *Uncommon Ground: Archaeology and Early African America, 1650–1800*. Washington, D.C.: Smithsonian Institution. 1992. Pp. xlv, 186. Cloth \$35.00, paper \$14.95.

Leland Ferguson's interpretive synthesis is an ambitious effort to explain African-American cultural development within the multiracial colonial plantation system that characterized early American colonial life. His theoretical framework rests on the creolization model formulated by the Jamaican poet and historian Edward Kamau Brathwaite and elaborated by historian and folklorist Charles Joyner. Ferguson's rendition of that model sees material things as part of the lexicon of culture, and the ways they are made, used, and perceived as part of the grammar or structure. His comparative investigation of the material remains of slave communities from Maryland to Florida uncovers a mingling of European, Native American, and African cultures to produce in the end not one but three creolized subcultures in southeastern America.

Ferguson seeks to describe and explain the complex process of creolization through an analysis of craft activities, culinary culture, architecture, and religion. His analysis of Colono-Ware artifacts from a dozen institutional repositories reveals divergent patterns, depending on the degree of interaction between the various cultural groups. The Colono-Ware and the clay pipes taken from the Pettus and Utopia sites on the Kingsmill tract in Virginia are a "creole production of mixed cultures" (p. 52), made with native technology but European in shape and decoration. Ferguson also sees "dual roots" in the hand-made pottery found at colonial sites in South Carolina, where most pottery remains have been discovered. The red painting and complicated or check stamping on some ceramics suggest manufacture by Catawba Indian potters, but the increasing dominance of plain bowls and jars with African features, which coincided with the growth of the African population, is a clear indication of African-American manufacture, an independent contribution for which Ferguson deserves full credit.

Ferguson's somewhat cursory survey of culinary culture suggests that a syncretic process linked the food ways of Europeans, Native Americans, and Africans through ingredients, cooking techniques, and eating habits. On the topic of architecture he adds little that is new to the extensive archaeological and historical literature on the subject of slave housing in early America. His discussion of Virginia slave housing as a composite of forms and techniques owes much to Mechal Sobel's meticulous analysis in *The World They Made Together* (1987), which described slave houses as European in basic design but with significant African innovations, among them porch additions and root cellars. Ferguson uses the work of Patrick Garrow and Thomas Wheaton to demonstrate how the demographic dominance of Africans in the low country translated into a more conspicuous accommodation of African styles of living. He finds such evidence in the rectangular shape of the thatched and clay-walled houses occupied by slaves of Vaughan and Curribo plantation on the Santee River in South Carolina and the similar dwellings at the Spiers Landing site farther up the Santee; in their division into two chambers in customary West African fashion; and in the use of dirt floors and exterior hearths.

Ferguson's treatment of religious practices and spiritual beliefs as they are revealed in the archaeological record is limited and disappointing, particularly so as the theme of African cultural assertiveness runs through the book. Although he deserves credit for providing the welcome information that the San Marcos pottery found in Florida reveals that Indian women were "major contributors to creolized St. Augustine society" (p. 40), there is no sustained effort to "engender archaeology," to borrow current terminology. Ferguson's attempt to recover African-American history from "stories in the ground" (p. 123) is, in short, a complicated and only partially successful undertaking. In large measure this is due to the interpretive complexity of the book, the various nuances of which cannot be adequately dealt with in 123 pages. Despite these shortcomings, Ferguson's book is a major analytical achievement. Through its bold proposition that slaves enjoyed "ideological power," it offers a new and different model for analyzing plantation social relations.

SYLVIA R. FREY  
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DANIEL H. USNER, JR. *Indians, Settlers, and Slaves in a Frontier Exchange Economy: The Lower Mississippi Valley before 1783*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1992. Pp. xvii, 294. Cloth \$32.50, paper \$12.95.

Daniel H. Usner, Jr., has achieved two goals much desired by historians of colonial North America: a

history of relations among Indians, Europeans, and Africans; and a history that brings the story of all three to bear on an important current debate, one that originated not in the subdiscipline of ethnohistory but in the social and economic history of early America. Participants in that debate over the "transition to capitalism" will here find the many peoples of the Lower Mississippi shaping, and affected by, economic developments. It is a striking achievement.

Usner traces the formation and decline of what he calls the "frontier exchange economy." The players included European planters, soldiers, sailors, and peddlers; African slaves, traders, and maroons; powerful inland Indian peoples, weak *petit nations*, and Indian slaves. Women produced and distributed important goods; they were not just consumers. By 1783, however, the "frontier exchange economy" was in irreversible decline, while a "commercial export economy," the foundation of the Old South, achieved dynamic ascendance.

In the first half of the book, Usner records the formidable turbulence of the region. The Lower Mississippi's cultural complexity attended a tumultuous political and diplomatic history. Political power changed hands with alarming frequency. The power and composition of Indian peoples underwent the fullest transformations; most declined, but some, like the Creeks, gained power over much of this period, strengthened numerically with refugees from devastated coastal regions. The French regime, meanwhile, remained weaker in Louisiana than in Canada. The lack of imperial interest and the low colonial population, much bemoaned by colonial officers and large planters, ensured the survival of a flexible and multicultural "frontier exchange economy." But with the close of the Seven Years' War, imperial claims to the region passed from France, to be divided between Spain and Great Britain. Both powers aggressively recruited colonists and intensified plantation slavery, giving new, and ominous, direction to economic relations among the region's peoples.

Following this overview Usner pays closer attention to several economic activities that characterized the "frontier exchange economy." Economic roles in this southern region only later became rigidly defined by race. In the early eighteenth century, blacks carried firearms, both for hunting and for war; Tunica and Avoyelle Indians traded in cattle and horses; black cowherds, even when slaves, achieved considerable autonomy; maroons rustled livestock; Indians worked in the colonies as free laborers or slaves. Africans, Europeans, and Indians alike engaged in hunting, farming, and herding, often borrowing techniques from one another. Through all these activities, the participants generally mixed their commercial activities with subsistence strategies, a pattern that both hedged small producers against disaster and distinguished the frontier exchange economy from a full-scale commercial economy.

Usner's description of the frontier exchange econ-

omy, hotly peppered with war, disease, and slavery, does not indulge in nostalgia. But it is with some sense of lost opportunities that he describes the transition to commercialism, imposed, in his view, largely from above. Usner's planters and merchants, with the help of colonial authorities, the military, and the law, gradually narrowed opportunities for small producers—European, African, and Indian. We might question Usner's characterization of the displaced economy's practices as "long established" by 1783, particularly in light of the period's turbulence and devastation, which generated considerable uncertainty, mistrust, and deception. Still, Usner's strong argument has great advantages: it draws our attention to the role blacks and Indians have played in the larger social history of the American small producer, and it promises to draw all the peoples of a neglected American region into the mainstream of scholarly debate.

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CARLA GARDINA PESTANA. *Quakers and Baptists in Colonial Massachusetts*. New York: Cambridge University Press. 1991. Pp. xii, 197. \$44.50.

Carla Gardina Pestana's welcome comparative treatment of Baptists and Quakers joins the many volumes on colonial Boston and Salem. Her study is sufficiently narrow geographically for both groups to permit detailed examination. That approach, however, prevents her consideration of both sects throughout Massachusetts as suggested by the title, especially after 1691.

Pestana's analysis of the first appearance and subsequent development of Baptists and Quakers adds substantially to our understanding of them and their relations with the Standing Order in this area, where their numbers were few compared to elsewhere in New England. Quakers were outsiders from the start; Baptists originated within the Puritan community. Both retained these positions in the eighteenth century, Baptists by coming closer to the established church as a denominational group, Quakers by reforms that preserved and enhanced their position as a sect. Throughout this period, Pestana argues, Baptists and Quakers differed in more than church polity. At first, Quakers were localized, focused on Salem. Baptists came from throughout the Bay Colony in the seventeenth century, reacting at first to changes within the dominant church. Quakers challenged the Standing Order by repudiating the reformed faith; Baptists found their inspiration in the reformed faith and developed even more restrictive membership requirements than Puritan churches. Pestana also discusses organizational developments, leadership, and assimilation. She concludes with an assessment of both groups in the era of the Great Awakening and the American Revolution.

Pestana's work has the advantage of a local study that permits her to follow churches and their members in communities. Thus, she is able to describe in detail the activities of many individuals, including William King among Quakers and John Farnum among the Baptists. She tracks leadership patterns and local secular activities in ways not possible in general studies; for example, in the cases of Elisha Callender and Jeremiah Condry she demonstrates convincingly the advancing intellectual and social status of Boston's Baptists.

A focused study like this can have its drawbacks when one considers the larger setting of Massachusetts Dissenters, especially those Baptists and Quakers who had been part of Plymouth Colony and with whom the Standing Order had to deal when Plymouth joined Massachusetts in 1692. The issue of taxes to support the Congregational church was important in Old Colony dissenting towns like Dartmouth. Curiously, Pestana does not deal with this subject extensively even in the vicinity of Salem, appropriately enough perhaps for Boston Dissenters who did not have to pay the church tax, but not for Quakers in Lynn, members of the Salem Monthly Meeting. These Friends were fined for refusing to pay church taxes and sent many reports of goods seized by authorities to the central decision-making group for Quakers in this area, the New England Yearly Meeting. Similarly there are problems with her assertion of the quasi-independence of Salem Monthly Meeting Friends. Local Quakers could and did move in independent ways, as Jean Soderland pointed out for antislavery activities of Friends in Pennsylvania and New Jersey (*Quakers and Slavery* [1985]). Salem Friends, however, decided to manumit slaves not because of a local decision to act independently, as Pestana asserts, but rather because of a nudge from the New England Yearly Meeting, which appointed a special committee to encourage Quakers from Boston north to catch up with other New England Friends. The northern Quakers had dallied on manumission, unlike Friends in the rest of the yearly meeting. Pestana could also have noted the extent to which the Salem Monthly Meeting relied on the yearly meeting to help build meeting houses in Salem and Boston. Clearly, this monthly meeting could not have functioned as effectively as it did without help from coreligionists in southeastern Massachusetts and Rhode Island. She also fails to convince when she argues that other scholars have failed to understand the changing nature of Quaker rules (the discipline).

These criticisms excepted, this is a model work that should inspire others to continue to examine communities where Dissenters were numerous. Some of Pestana's conclusions could be investigated in another setting. For example, were Baptists in southeastern Massachusetts as dispersed as in the vicinity of Boston? Did Quaker leadership elsewhere in New England depend on kinship ties rather than economic

and social standing in a community? Were Quakers elsewhere clustered as in the vicinity of Salem?

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HARRY S. STOUT. *The Divine Dramatist: George Whitefield and the Rise of Modern Evangelicalism*. (Library of Religious Biography.) Grand Rapids, Mich.: William B. Eerdmans. 1991. Pp. xxiv, 301. \$14.95.

We have had trouble keeping George Whitefield in focus because we have had trouble keeping eighteenth-century Anglo-America in focus. How religious were those people? Did a "Great Awakening" sweep unchurched colonists into the churches, thus creating a "Christian" people ready to fight a holy war against England to save their Christian liberty? Did the "Great Awakening" even happen? The battle lines are drawn and the historians are lined up neatly in battle order.

Harry S. Stout believes that Anglo-Americans were pretty religious, the Great Awakening did happen, and the Great Awakening did create a common "American" (although not exactly Christian) culture capable of war against England. Moreover, he believes that George Whitefield was a major figure in bringing all of this about. Thus, Stout's biography is not exactly groundbreaking revisionist history, but it is the best book we have about the great evangelical, and it provides a very different portrait of Whitefield and of Anglo-America than one finds in Stout's previous book, *The New England Soul* (1986).

In this volume, Whitefield is a "modern" person literally "peddling" an old message. He is a drummer, and his product is the old Calvinist line about sin and the absolute need for a "new birth." Whitefield takes the message out of the context of the local church and the local community and goes on the road, at about the same time that American "consumers" become wedded to English goods (mostly bought on credit) and peddlers begin leaving Kensington, Connecticut, to sell tinware all over Anglo-America. Commerce and the market were transforming the lives of the colonists. The colonists were being Anglicized, forced to become distant and inferior participants in a transatlantic culture and empire dominated by England and London.

Whitefield was an actor whose "script" emphasized emotion and feeling, not theological niceties. Whitefield learned the actor's craft and wrote his own monologues; literally doing "plays" (although he lambasted the theater) in which he acted out the biblical lives of the saints. His medium was emotion and his message was a "new birth," which might come anywhere, anytime. He adapted business and market practices by endlessly and shamelessly promoting himself and his "show." He knew that communal loyalties were dying and that outlanders wanted the news, the products, and the religion of the metropole.



He knew that in religion, just as in tinware, people would abandon the local merchant when they heard the call of the drummer. He discovered the secret and passed it on to Jim and Tammy, Pat, Jerry, and Oral.

So Anglo-America in the eighteenth century became part of a single culture directed and controlled from London, and Whitefield was more important to the creation of that culture than any general, governor, or merchant. But how are we to understand the emergence of a distinctive Anglo-American culture and Whitefield's eventual defense of colonial rights? We do not, fully. But we do see something that may have eluded even Stout. Whitefield spent his career in England, Scotland, and Anglo-America. He chose Anglo-America as his favorite. Why? Probably because of the lack of an ecclesiastical structure in America. In England and Scotland he was tripping constantly over bishops and synods. He had no such problems in America. American ecclesiastical institutions were weak (as were most other institutions) and Whitefield could have his way. He had to worry about personalities, not institutions, and when a minister gave him trouble he swept him aside by appealing to the crowd. It worked and he loved it. It still works, and the George Whitefields of the land still love it.

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JOHN CORRIGAN. *The Prism of Piety: Catholic Congregational Clergy at the Beginning of the Enlightenment*. (Religion in America Series.) New York: Oxford University Press. 1991. Pp. x, 197. \$29.95.

Scholars of New England's history fascinated by Perry Miller's brief but suggestive treatment of such figures as Benjamin Colman, William Brattle, and other "liberal" clergymen (*The New England Mind: From Colony to Province* [1953]) will welcome this study by John Corrigan. The narrative of this period usually focuses on the Mather family and Solomon Stoddard, but Corrigan shifts the lens to the complex ways in which Enlightenment thought influenced New England's theology and culture through the above-named ministers and their cohort—Benjamin Wadsworth, Ebenezer Pemberton, Edward Holyoke, and a few other influential individuals. These "catholic" clergy welcomed the new developments in science and religion that marked the Enlightenment and willingly adapted their Puritan theology to these developments. They have long warranted such detailed attention.

Influenced in their religious thought by such latitudinarians as Edward Stillingfleet and John Tillotson, among others, these clergy were more willing than many of their contemporaries to tolerate differences of opinion in religion. Moreover, early on they also absorbed the ideas of such thinkers as Robert Boyle and John Ray, more openly devoting them-

selves to the study of man and nature and delighting in the order they detected in the universe. Concomitantly, for them conversion implied "being drawn to God's love" more than being "chased to him in fear" (p. 7), for the creator of such an orderly world was not to be held in dread. Finally, because they were so interested in man and his "affections" (which they understood as originating in both body and soul), their piety was both highly sacramental, as Brooks Holifield has demonstrated (*The Covenant Sealed* [1974]), and affective, binding them to other Christians in a kind of public religion. In short, these are not the sort of people we usually associate with the waning days of New England Puritanism, but they were indeed a significant leaven in it.

Corrigan details such matters and others in his study. Beginning with a discussion of English influences on his clergy, Corrigan also offers close readings of their many published works. But we miss the grandeur and excitement of Milleresque narrative; Corrigan fails to make the ideas come to life, to show how they mattered in some visceral way. Thus, this little book seems more like an extended footnote than an engaging study, a particularly ironic fact when one realizes that virtually one-third of the book itself consists of footnotes.

Closely related to *The Hidden Balance* (1987), Corrigan's work on Jonathan Mayhew and Charles Chauncy, this book helps us to understand the way in which the Enlightenment came to America, as Corrigan puts it, "as a steady process of change, rather than a wholesale exchange of one set of ideas for another" (p. 134). This is salutary to recall, and those who wish to explore in more depth the Enlightenment in America will find this book useful.

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RUSSELL E. RICHEY. *Early American Methodism*. (Religion in North America.) Bloomington: Indiana University Press. 1991. Pp. xix, 137. \$25.00.

It is quite an achievement when a historian reports much historical development without bearing it along by narrative flow, builds a strong hermeneutical argument without theory impeding the telling of the story, and when revisionist endeavors avoid slipping into preoccupation with revisionism. Russell E. Richey manages in all three respects. His book abundantly informs the reader about what was occurring in the Protestant movement, exported from John Wesley's Britain to the American colonies, that took root and shape between 1780 and 1810. He also exploits to great advantage the trope of four Methodist "languages"—evangelical, Wesleyan, episcopal, and republican—a creative technique that illuminates Methodism's component parts and the problematic of

their correlation. Finally, he corrects many distortions that have obscured our understanding of the Methodist movement, doing so only as a consequence of assiduous work in the primary documents and the received interpretations.

Listing only a few of Richey's revisionist findings makes the point. The circuit-rider more often rode with others, in a kind of "seminary on horseback" (p. 10), than he forayed in barren, stoic solitude. "Camp meetings" were less often excrescences of the "frontier" than planned occasions of a settled and quite deliberate religious body. Early Methodism is not accurately described as an accommodating, culturally captive product of the vaunted "Americanization" process. And the itinerants less often "won the West" than they nourished the religious life of Methodists who had moved from such places as Virginia and Maryland to Ohio, Tennessee, and Kentucky.

Rarely does such economical prose convey so many insights and generate so much historical and historiographical rumination. The series editors claim that Richey's accomplishment is the best "account of organized religious activity" (p. viii) since Rhys Isaac's book, *The Transformation of Virginia, 1740–1790* (1982). Indeed, this study is more cautious than Isaac's while being as innovative and thoroughly nuanced.

No theme in this book impresses more deeply than Richey's contrasting of Methodism's "public policy" (the "republican language") with that of the Reformed-Calvinist-Puritan branch of colonial Protestantism. The latter yearned for theocracy in one form or another, aiming to build a godly state and thinking in terms of God's covenant with a specially designed and called America. Methodism, Richey argues, came quite late to advocating "Christian republicanism." In the early decades it concerned itself far more with American "lands," "territories," or the American continent than with polity or civitas. Its focus fell on the spatial, not the political.

Scholars can imagine the personal debt they incur from Richey's teaching about the field of southern religious history. He makes a compelling case for the creative role of the South in shaping American Methodism, its "southern accent." He contends, beyond the work of any veterans in the field, that "southern religion, at least in its Methodist form, may be difficult to distinguish because it imprinted American Methodism as a whole" (p. 50).

Denominational history has not been popular for producers or consumers for several decades. Some would argue that it rarely has sparked or led the way in the study of religious history. Richey's work alters all such responses. This is a profoundly effective study, in denominational history all right, but done with a sophistication and general applicability that gives new life and direction to that old enterprise.

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ELAINE FORMAN CRANE, editor. *The Diary of Elizabeth Drinker*. Volume 1, 1758–1795; volume 2, 1796–1802; volume 3, 1803–1807. Assisted by SARAH BLANK DINE *et al.* Boston: Northeastern University Press. 1991. Pp. lxxv, 766; 767–1611; 1612–2398. \$210.00 the set.

The publication of a three-volume, scholarly edition of the diary of Elizabeth Drinker (1735–1807) is an important event in the historiography of the American Revolution and early national era. The diary, which spans the years 1758 to 1807, is the most substantial woman's diary that survives from eighteenth-century America; and it ranks with the diaries of Samuel Sewall, William Byrd, Landon Carter, John Adams, and William Bentley in its richness as a source for understanding the social and cultural history of the period it covers. Although the Drinker diary has long been known to scholars and been available for fifty years in a WPA typescript, Elaine Forman Crane and her associates' letterpress edition is a dramatic advance. Not only is the text rendered accurately, but it is indexed for around 1,500 names (my estimate) and a like number of subjects. Moreover, the editors have supplied a 137-page biographical directory that identifies most of the individuals named in Drinker's text. So that readers may fully understand Drinker's entries, the editors' unobtrusive scholarly notes provide information about events that are mentioned in the text and they also supply full bibliographic citations for the many and diverse publications that Drinker noted. In addition, a series of genealogical charts enable readers to trace the complex web of connections among Drinker's extended family. As an editorial achievement, Crane and her associates' work is a triumph that fully justifies the support that the National Historical Publications and Records Commission, Fordham University, the Barra Foundation, and others have given to the project.

The diary itself is uneven in its chronological coverage, reflecting in part, perhaps, the amount of leisure time Drinker had available. From 1758 through 1776, when she was in the midst of child-bearing and child rearing, her diary averaged only twelve printed pages per year, and there were many chronological gaps. By contrast, from 1794, when she was 59 years old, until her death in 1807, her diary averaged over 100 printed pages annually. As a source, the text makes its richest contributions for the years 1777 and 1778, when it supplies an unparalleled record of civilian life and loyalist perceptions in wartime Philadelphia, and in the dozen years beginning in 1793, when Drinker's entries touch a wide range of social, cultural, and political subjects.

Drinker's voice, as Crane points out in her informative biographical introduction, was intentionally unemotional and almost uniformly matter-of-fact. She adhered to a genteel eighteenth-century standard of controlling emotion that is reflected in nearly all the diaries, male and female, of the period. Sig-

nificantly, although Drinker read novels and "romances" on occasion, she was ambivalent about using her time in this way, since novel reading was, she said, "a practice I by no means highly approve" (p. 769). As family responsibilities permitted, Drinker read extensively; but as one untutored in critical analysis and reared in Quaker humility, she was reluctant to express her own opinions at length, and often only recorded the titles of the books, pamphlets, and newspapers that she read. One of her fullest assessments was of Mary Wollstonecraft's *A Vindication of the Rights of Woman* (1792): "in very many of her sentiments, she, as some of our friends say, *speaks my mind*, in some others, I do not, altogether coincide with her—I am not for quite so much independence" (p. 795). Her judgment on Thomas Paine's "vile writings" (p. 840), "poisoned discourses," and "blasphemies" (p. 1050) in *The Age of Reason* (2 vols., 1794, 1796) was unequivocal. In contrast, she offered no comment at all when she finished reading a translation of Dante's *Inferno*. As a result the diary does not permit readers to enter fully into Drinker's active intellectual life, although it does supply a record of an aging woman's remarkably capacious reading interests.

The aspect of the diarist's mentality that is most fully revealed is her intense involvement with family members, their comings and goings and their health. A by-product is a full record of practical medicine in Philadelphia during ordinary as well as yellow fever epidemic years, including traditional remedies, advice gleaned from British medical manuals, and the prescriptions of such learned physicians as Benjamin Rush. Readers also gain entry into a world of sociability in Drinker's detailed record of visits made and received. Information concerning domestic labor relations as well as race and gender relations is also supplied in passing.

For scholars interested in Philadelphia and its leading Quaker families, the diary provides rich veins to mine; but it would be a mistake to view the Drinker diary as chiefly of interest to local or sectarian history. In fact this diary has an encyclopedic topical range, most of which is accessible through the indexes and its chronological organization. As a result everyone who has an interest in the history of early America will be instructed by going to the diary, and every college and university library will be wise to add this fundamental text to its collection.

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GARRETT WARD SHELTON. *The Political Philosophy of Thomas Jefferson*. Baltimore: Johns Hopkins University Press. 1991. Pp. x, 174.

This book establishes systematically, and I think conclusively, that there was in Thomas Jefferson's polit-

ical philosophy a large measure of "classical republicanism." Garrett Ward Sheldon shows repeated, unmistakable parallels and congruences between Jefferson's writings and those of Cicero, James Harrington, Montesquieu, Rousseau, and especially Aristotle (but he excludes explicitly Machiavelli from this group). In accepting that "man is a political animal," in seeing active, public-spirited citizenship as essential to human fulfillment, in endorsing the vital educational role of the community, and in seeing self-government as a deliberative, rather than adversarial, process, Jefferson revealed his basically Aristotelian understanding of politics and public life. His great insight and creativity was to see how this perspective could be complemented by a Lockean idea of natural rights and limited government and then applied to the unique circumstances of the founding of the United States. Although the argument on behalf of classical republicanism has been at least implicit in dozens of books of the last twenty-five years, Sheldon puts the case for Jefferson's inclusion more clearly and thoroughly than we have seen it before.

Sheldon makes an unnecessary and problematic argument, though, that Jefferson was largely Lockean (or "liberal" in modern usage) as the author of the Declaration of Independence resisting British tyranny, then classically republican in helping found the new nation, but again Lockean late in life to oppose what he saw as the Federalist, or northern, or commercial dominated power of the central government. This interpretation makes some sense as a matter of emphasis—Locke's argument about the right of revolution was especially relevant in 1776, for example—but Jefferson's genius was not in shifting his political thinking to adjust to immediate needs; rather it was in fashioning a synthesis that could carry him through rebellion, establishing government, and responsible opposition. He believed in an objective standard of a just and good society that could at once validate rebellion, be a basis for establishing government, and undergird civic education. The foundation of this standard rested on both right reason and the "moral sense" of humankind. Jefferson drew this outlook from what he termed the elemental books of public right by Aristotle, Cicero, Locke, and Algernon Sidney, thus sustaining a balance in his mind of classical and modern patterns of thought as apparent in 1776 as in 1806 or 1826.

The book is for the most part admirably concise in its argument, yet a preoccupation with secondary scholarship detracts from what would probably be a more interesting and incisive attention to Jefferson's own words. A beginning chapter and an appendix that each survey the already over-canvassed scholarly debate between the "classical republican" battalions led by J. G. A. Pocock and the "liberal" forces led by Joyce Appleby is redundant and tiresome. The discussions of the Louisiana Purchase, the embargo, and "the Shadow of Slavery over Jefferson's Political Philosophy" (pp. 129–40), depending on very narrow

ranges of secondary works, seem incomplete and even superficial. Nonetheless we are indebted to Sheldon for a learned, lucid, and generally sound explication of Jefferson's political thought.

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GORDON S. WOOD. *The Radicalism of the American Revolution*. New York: Alfred A. Knopf. 1992. Pp. x, 447. \$27.50.

This is a beautifully crafted book. Gordon S. Wood has divided his study into three sections: monarchy, republicanism, and democracy. As the rubrics suggest, he interprets the revolutionary years according to their dominant political form, republicanism bringing an end to monarchy only to be quickly overtaken by democracy. To capture the cultural essence of his sequential social forms he creates discursive collages of anecdotes, quotations, and illustrative details, adroitly arranging them to show us how sensibilities, values, and understandings of reality changed under the pressure of events with which the reader is presumed to be familiar.

Wood's artistry is most apparent and most effective in the opening section on monarchy. One can imagine avid readers pouring over these pages for another century, entranced by the connection of the United States with a world that we have not only lost but have never really claimed as our own. The ramifying importance of family, status, and patronage stands forth clearly as Wood summons witness after witness to tell about this most untraditional traditional society.

But Wood aims at more than the presentation of enduring cameos of early America. He has a strong, if not startling, thesis that is expressed in the quaintly eighteenth-century subtitle, "How A Revolution Transformed a Monarchical Society into a Democratic One Unlike Any That Had Ever Existed." For Wood, the "revolution was the source of its own contradictions" (p. 230). These contradictions surfaced quickly after 1776 and played themselves out by sweeping aside first the unthinking grace of an aristocratic way of life and then the thoughtful plans of the founders' utopian republicanism, leaving to thrive without contest the rambunctious, money-mad, interest-fueled democracy of nineteenth-century America.

In what might be considered a reprise of Wood's justly famous first book, *The Creation of the American Republic* (1969), the high drama of this study comes when rude reality intrudes on the "classical republican dreams of establishing a government led by disinterested educated gentlemen" (p. 259). The fight against Britain stirred the political imagination. "Suddenly, in the eyes of the revolutionaries, all the fine calibrations of rank and degrees of unfreedom of the traditional monarchical society became absurd and

degrading. The Revolution became a full-scale assault on dependency" (p. 178). A new goal provoked a new vision. "At the height of the patriot frenzy in 1774-76 many of the revolutionaries wanted nothing less than a reconstruction of American society" (p. 213). Enlightenment hopes for the civilizing effects of natural sociability made the republican revolution "the greatest utopian movement in America." But time would not stand still for the cultivation of modern civic humanism. Humming in the background of American society as well as behind Wood's analysis were "the same dynamic forces at work since the middle of the eighteenth century—population growth and movement and commercial expansion" (p. 308). The conflict that began by pitting patriots against courtiers transmogrified into a contest between democrats and aristocrats, only to end in the Jacksonian era with a consensus that harmony could and would emerge from the scramble for wealth from a populace of single-mindedly ambitious individuals. Left to lament their revolution's success were the aging revolutionaries themselves (pp. 241, 359).

Despite the centrality of the revolution, it frequently disappears while Wood describes the larger social transformations occurring between 1740 and 1820. The book's strength lies in the subtlety of the analysis of the interacting effects of such things as demographic expansion, an intensifying commercialization, the intrusive semiotics of contract, and popular encroachment on elite political preserves. The why behind the transformation is less clear. The most salient cultural changes—whether concerns about idleness, the reworking of the parent-child relationship, the heightened significance of being a gentleman, or the elaboration of consumer demands—began in England, and hence cannot be attributed to the revolution. Yet whatever the origin, the impact fell heaviest on colonial America. It is as though the great ship, Britannia, hit turbulent, uncharted waters that capsized the skiff, America, following along in its wake. Elsewhere Wood yields to another implication of his account when he writes that the "imperial crisis with Great Britain and the American Revolution itself were simply clarifying incidents in this larger story of America's democratic revolution" (p. 125). Here is the acorn of Tocquevillian equality finally grown into the populist oak in whose shadow other political principles must wither. Wood does not tell us which metaphor is more apt, but he brilliantly illuminates and sympathetically details the elements at play in the American metamorphosis from colony to nation.

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JAMES TAGG. *Benjamin Franklin Bache and the Philadelphia Aurora*. Philadelphia: University of Pennsylvania Press. 1991. Pp. xiv, 431. \$38.95.



This is the second book in as many years to be published about Benjamin Franklin and his grandson, the publisher of the Philadelphia *Aurora*, who died after a brief, mercurial career at age twenty-nine in the yellow fever epidemic of 1798. (Jeffery A. Smith's *Franklin and Bache: Envisioning the Enlightened Republic* was published in 1990.) It should hardly surprise us that scholars are focusing on the decade of the 1790s, when republicanism began to transform itself into democracy, or that the career of the editor and publisher of the most important Republican newspaper of that decade should attract the attention of those seeking insight into the dynamics of such a fundamental transition. Nor, given Franklin's influence over Bache's education and career, is it surprising that James Tagg's book focuses almost as much as its predecessors have on the grandfather as on the grandson. But where others have seen Bache as an extension and fulfillment of Franklin's philosophical commitments, Tagg portrays a more complex psychological relationship between the two.

In assessing the influence of family dynamics on Bache's development, Tagg stresses the "fragmentation and deep divisions [that existed] between the generations" (p. 16). This fragmentation was accentuated when Franklin took Bache, aged seven, to Europe in 1776. Bache was relegated to the margins of Franklin's busy life, first being placed in a Parisian school and then, as a republican corrective, in a Genevan one. Tagg feels that Franklin was "attentive to Benny's physical needs but neglectful of . . . his emotional ones" (p. 35). Far removed from family and friends, Bache had an especially difficult two years in Geneva, learning even before adolescence "to steel himself not to show dependence and need" (p. 39). Eventually he emerged from a long period of loneliness and depression with a strong sense of "moral self-reliance and true autonomy" (p. 41) that allowed him to deal successfully with his imposing grandfather. But Tagg believes that it was a costly liberation based on "a premature autonomy," and that it provides a psychological clue to "the force behind [Bache's later] radical philosophical and political commitment" (p. 49). Tagg argues that Bache stopped developing in his adolescence and became imprisoned in an "ideological mind" (p. 80) that he failed to transcend in his mature working life.

Tagg devotes the rest of the volume to Bache's career as the publisher of the nation's first daily, which was launched in 1790 as the *General Advertiser* and eventually transformed itself into the *Aurora* in 1794. The book's strength lies in the exhaustive account it gives of the politics of the 1790s. Every twist and turn of events in that complicated decade is examined not only from Bache's perspective but also in the context of the secondary literature dealing with the period. Because Tagg scrupulously respects the authority of the *Aurora*, in several respects his account provides a useful corrective to deficiencies that exist

in the monographic literature. Tagg has failed, however, to solve satisfactorily an internal problem in his argument growing out of the tension between the psychological analysis of Bache's childhood and adolescence and what follows thereafter.

The tension is twofold. First, Bache started the decade as a moderate, almost antiparty editor. He only moved toward partisanship in 1793, and he only became stridently ideological in response to the Jay Treaty. The attempt to attribute his eventual extremism to arrested personality growth cannot account for why he did not behave in a more partisan way earlier, particularly when others were doing just that. Second, when Bache moved into his rabid phase he had a great deal of company, particularly in the Jay Treaty controversy. Are we to believe that all these people suffered from fragmented families and premature autonomies? Bache played a more important role than others in the ensuing partisan struggles and it could therefore be argued that his personality significantly affected the flow of events. Yet most commentators on this period have been struck by the pervasive dementedness of the period's politics. This suggests to me that we need a more widely shared attribute to explain the character of the age than individual personalities that are inevitably idiosyncratic.

The most promising approach to understanding the peculiarities of the 1790s remains a focus on ideological issues. Cultures that undergo revolutions, as America's had recently done, have reasons of their own quite independent of personality for ideological sensitivity. Tagg acknowledges the importance of ideology, but he attaches more significance to Bache's ideological particularities than the period's ideological common denominators. Bache is portrayed as a disciple of Tom Paine, a modern radical—legitimizing self-interest through his vision of politics as the expression of will—implicitly opposed to the values of classical republicanism. But surely there were more urgent matters on the ideological agenda of his era than this rather academic sounding dispute. Given the earthshaking character of the French Revolution, defending the dignity and viability of republicanism in an extraordinarily threatening world surely took precedence over whether one was a backward-looking or forward-looking republican from the perspective of the twentieth century. Emphasizing what Bache shared ideologically in common with others would have had the tendency of reducing him from influence to exemplar. But it is hard to read Tagg's account without coming to the conclusion that Bache's attention was principally focused, as was that of his contemporaries, on the international pressures affecting the future of republicanism. From this perspective Bache's primary significance lay in helping to find a republican way to respond to these perplexing pressures and their domestic ramifications rather

than ushering in a new, more radical version of republicanism itself.

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SUSAN L. PORTER. *With an Air Debonair: Musical Theatre in America, 1785–1815*. Washington, D.C.: Smithsonian Institution. 1991. Pp. xiv, 631. \$55.00.

Any reader should be pleased with Susan L. Porter's rundown of musical theater in America from 1785 to 1815. In the interest of assessing all the aspects of theater production in that period she uses much of the material available in the early histories of the theater—William Dunlap's, Joseph Ireland's, and George Seilhamer's—as well as later ones, such as G. C. D. Odell's day-to-day account of the stage in New York, Oral S. Coad and Edwin Mims's largely biographical survey, and Arthur Hobson Quinn's work on American drama. These studies date either from the nineteenth century or from the 1920s, but to give us a fresh view of theater managers, actors, programming, and staging, she goes back to the primary sources of newspapers and journals and, in addition, takes advantage of recent work on the period. By gathering this material under subject headings, she has avoided the dullness of chronological listings.

But Porter's study goes much further than this. What is really new and what makes the book most useful is her exploration of theater music of the period, a job she is qualified to do as a music historian. She is thus able to show how music was tied intimately to play production, as well as how it held the stage in its own right in operas, concerts, and whatever musicians could put together for an evening's entertainment. All in all, Porter presents a composite guide for graduate students in either theater or music history and provides an entertaining overview of the period for the casual reader.

By confining her study to the period 1785–1815, Porter is able to concentrate on imports from the English stage. As she notes, indigenously American materials did not begin to find focus until later, a date I placed close to 1825 in my study of American comedy (*Yankee Theatre* [1964]). For the background in music in my study I depended largely on Oscar Sonneck's detailed look at early American music, also Smithsonian publications. Now Porter has augmented Sonneck's work by providing a rich composite of the imported music from the London stage, along with discussions on the training of the vocal instrument and vocal ornamentation. Her details on theater orchestras as they existed in the major theaters makes a fresh report. Although one could debate her assertion that music played a "central" role in the success of the early theater in America, music has usually been undervalued by theater historians.

Porter's chapters on the training of actors and singers are particularly revealing.

Probably most useful to the advanced scholar of the period are the appendixes, which feature an exhaustive check list of musical entertainments performed in America from 1785 to 1815; a listing of musical theater performances in New York, Boston, Philadelphia, Baltimore, and Charleston; and an extensive set of footnotes in support of her text. Taken together, they reveal the careful scholarship in this study and how much it relies on the contemporary newspapers and journals of that period.

For anyone who has not read much about theater organization and production during this period, Porter has provided a most readable account of the wide activities of the imported theater from London and how it made its way in America. Although the suggestions on how to stage the materials of this period for twentieth-century audiences might be misleading, they still serve a useful purpose in encouraging revivals.

In publishing this work the Smithsonian Institution Press has added to its fine collection of in-depth studies that provide a genuine service to the reader intent on discovering America's heritage in the arts.

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ELIZABETH JOHNS. *American Genre Painting: The Politics of Everyday Life*. New Haven: Yale University Press. 1991. Pp. xvi, 250; 80 plates. \$40.00.

Calling her book "an interpretive essay and not a monograph" (p. xiv), Elizabeth Johns does not claim to have written a comprehensive history of American genre painting. She excludes, for instance, representations of Native-American life and literary genre paintings in order to focus on those paintings that more directly defined the citizenry or body politic of the United States during the three decades prior to the Civil War. Paintings of specifically American subjects flourished within a fluid economic and cultural environment, fueled by the collapse of traditional hierarchical relationships and the rise of an aggressive, but insecure, middle class. Painted for an eastern, primarily New York, urban elite, representations of American social types were anything but innocent. According to Johns, antebellum genre paintings emerged from a cultural context of jokes and condescending storytelling that stigmatized certain groups as marginal or unfit for an active role in politics. Distinguishing the "other" by region, class, gender, and race or ethnicity, genre paintings tended to naturalize or mask this ideological typing through the employment of a realist technique.

Johns diverges from traditional art history by subordinating her analysis of artists' styles to an anthropological and historical deconstruction of these sup-

posedly reportorial images. She considers the paintings not as reflections of reality but as cultural constructions interconnected with a variety of visual and verbal discourses. The painted images and their interrelated discourses were not the focus of consensus but rather of conflict between competing social groups. Far from celebrating everyday life and the generic common man, these seemingly benign and often placid paintings expressed the deep-set anxieties and prejudices of both artists and patrons.

Arranging chapters according to the representation of key social types, Johns first examines images of the ungentle and scheming Yankee farmer, best represented in the paintings of William Sidney Mount. She argues that Mount's comic pictures enabled wealthy New York patrons to release tensions over their own self-interested quest for socioeconomic power. In responding positively to a Mount painting such as *Raffling for the Goose* (1837), New York merchants and real estate speculators could transfer moral guilt from their own circles toward a diminishing regional group that humorously typified economic selfishness in violation of the republican ideal.

Johns also examines other representations, including the Anglo-Saxon westerner depicted for eastern men seeking to control the economic exploitation of the West under the guise of manifest destiny; the child-like African American, exploited, not assisted, by abolitionist troublemakers; and the domestic woman, like the African American, represented without authority in the body politic. The last type that Johns considers, the street urchin, a metaphor for urban crime, was rarely depicted, despite the career of Pittsburgh artist David Gilmour Blythe, who specialized in representations of unwashed delinquents. Johns suggests that most artists, working for a more sophisticated New York market, found such subjects unsuitable for art. Genre painting in general struggled against a growing demand for more ideal, less provincial art. After the Civil War, with the political structure firmly ordered by increasingly complex bureaucracies and social hierarchies, art patrons could turn outward toward competition with Europe and inward toward the aesthetic refinement of a private, domestic culture.

Johns's readings of paintings are nuanced and open, underscoring her thesis that these images were the loci of ideological conflict rather than national consensus. Her interdisciplinary revisionism allows the pictures to speak vividly, although not with a unitary voice nor with messages that disinterestedly harmonize with the aesthetic emotion.

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CHARLES SELLERS. *The Market Revolution: Jacksonian America, 1815-1846*. New York: Oxford University Press. 1991. Pp. 502. \$35.00.

In the last two decades, few periods of American history have attracted more scholarly interest than the early republic. Charles Sellers draws on this recent literature in a wide-ranging, provocative reinterpretation of these critical years.

For Sellers, the defining drama of this era was the seizure of the "American future" by "history's most revolutionary force, the capitalist market . . . from history's most conservative force, the land" (p. 4). Sellers asserts that this fundamental struggle "has eluded or baffled our historiography of consensual, democratic capitalism" (p. 6). With characteristic determination, Sellers resolves to set the record straight. A "stressed and resistant Jacksonian majority," he explains, "rallied around enduring human values of family, trust, cooperation, love, and equality" as it battled to maintain its culture of land against the coming culture of the market (p. 6).

Sellers begins his account of this momentous conflict with a vivid description of the subsistence culture of early nineteenth-century America. He then details the expanding market's disruption of that culture in both rural America and the urban world of the artisan class. He focuses particular attention on the transportation, monetary, and legal revolutions that stimulated and accompanied the new nation's explosive market growth, and then attempts the mammoth task of demonstrating the extraordinarily widespread impact this growth had on American life.

Indeed, few aspects of Jacksonian culture and society escape Sellers's analysis of the market revolution's reach. Arguing that "class imperatives were experienced culturally and psychologically," Sellers maintains that the religious life of the new republic reflected the struggles initiated by the market's expansion (p. 215). New Light, evangelical religion, he explains, engaged followers in a flight from the "world's estranging competition" in what became a "massive cultural mobilization against the market and its ways" (pp. 159, 161). At the same time "market necessity generated for the more enterprising a Moderate Light tenuously blending the self-discipline of arminian effort with antinomian love" (p. 203). The resulting conflict between New and Moderate Light for the "soul of the universal Yankee nation" was resolved, Sellers concludes, in Charles Finney's revivals, which made "capitalism and an antinomian populace safe for each other" (pp. 218, 225).

In Sellers's view, the changing nature of religious commitments offers only one example of the market's impact on Jacksonian America. Antislavery, temperance, public schools, and the private reading of novels are just a few of the many, varied features of Jacksonian society that he claims owed their origin to the "market's cultural offensive" (p. 269). Venturing into the realm of private concerns, he cites cultural restraints on sexuality and a widespread denigration of eroticism and concludes that "through collective repression . . . middle-class society disciplined recalcitrant inclinations to capitalist effort" (p. 259).

The market revolution Sellers describes was, of course, a national phenomenon. The South was as deeply involved as the rest of the country. Reiterating a position he has taken for some time, Sellers warns us not to be confused by the "moonlight-and-magnolia haze of a mythical planter aristocracy" (p. 279). The South's planter class, he insists, was "fiercely entrepreneurial" (p. 279). Far from being "seigneurial in values and style," planters were themselves politically vulnerable to a "precapitalist farmer majority" (p. 279).

Sellers focuses most intently on resistance to the market and concludes that the "subsistence culture's most enduring legacy" proved to be the political democracy that this resistance produced (p. 31). "The democratic impulse," he explains, "was driven by feelings of insecurity and powerlessness as the market disrupted ordinary lives" (p. 32). Entrepreneurial elites needed state aid to promote economic development and "democracy promised farmers protection from [an] intrusive government" willing to implement a program of market expansion (p. 32). Sellers is at his best when conveying the detail, drama, and ambiguity of the democratic revolt against this program from its origins in Jeffersonian America to its culmination in the Jacksonian antibank struggle. With great care and sensitivity, he weaves both personality and circumstance into his interpretive narrative, expertly bending individual idiosyncrasy to his own analytic purposes.

Throughout, Sellers persists in the belief that politics reflected popular anger over "hard times." Shock over the boom-bust cycle of economic disruption ignited a new democracy that was "aggressively egalitarian in style and antidevelopmental in substance" (p. 165). This insurgent democracy, he maintains, joined the eastern working class with western farmers in a new class-based politics. Both groups searched "for good men who could be trusted to defeat the selfish stratagems of market elites" and concluded that no leader could be trusted more than Jackson, who, in Sellers's telling, rapidly became the hero of those opposed to the "menacing economic and cultural pretensions of the North Atlantic market world" (pp. 165, 179).

For Sellers, the triumphant climax of this drama was unquestionably the Bank War, with Jackson leading the crusade against the "activist capitalist state" (p. 331). The Bank War, he insists, was the "acid test of American Democracy. Never has the farmer/worker majority given a more radical mandate to a more indomitable President . . . Never has the majority seemed so close to actually ruling" (p. 321). Strenuously arguing the case for the purity of Jackson's "hard money" commitment to an independent treasury, Sellers concludes that Jackson "challenged bourgeois hegemony like no other President by assailing credit, the lifeblood of escalating enterprise" (p. 333). He admits sadly that the Bank War's conclusion in the election of 1840 "crystallized a politics that

has ever since muffled the contradictions between capitalism and democracy in a mythology of consensual and democratic enterprise" (p. 363).

Not since Arthur Schlesinger, Jr.'s *Age of Jackson* has this period received such an avowedly class interpretation. Indeed, Sellers himself suggests that "Schlesinger may yet be judged more nearly right than his critics" (p. 439). Sellers's views themselves are certain to encounter strong criticism. Few books have attempted so much and few have offered such an all-embracing explanation for so diverse a range of phenomena. Yet this study clearly will remain one of the central works of Jacksonian interpretation. Future examinations of the new nation will have to come to terms with this challenging volume.

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HARRY L. WATSON. *Liberty and Power: The Politics of Jacksonian America*. (American Century Series.) Paperback edition. New York: Noonday. 1990. Pp. xii, 275. \$9.95.

Harry L. Watson, the author of a valuable study of antebellum politics in North Carolina (*Jacksonian Politics and Community Conflict* [1981]), has now produced a well-written short history of the politics of what he thinks is rightly regarded as the Age of Jackson. It is no small compliment to report that his book puts one in mind of Glyndon Van Deusen's *The Jacksonian Era* (1959), published a biblical generation ago.

Both books are the work of scholars with an impressive command of the scholarly literature. Each of them focuses on national politics, on the great party battles fought out in the nation's capital. They highlight the dramatic set pieces of the political wars—Andrew Jackson's veto of the bill to recharter the second Bank of the United States, the nullification controversy, the "Eaton Malaria," the Maysville Veto, tariffs, and the American System. Emphasis is placed on the Great Men of politics, Jackson, Henry Clay, John C. Calhoun, Daniel Webster, Martin Van Buren, John Quincy Adams. The short but deft chapters on social, economic, intellectual, and religious developments serve primarily as background to the political themes that are at the core of both books. And both are moderate in tone, happily lacking in the stridency and rancor that has at times infected the modern scholarly discussion as it once permeated antebellum politics. These similarities in the two books are particularly interesting in view of their sharply different appraisals of the period.

Where Van Deusen agreed with Alexis de Tocqueville that Whigs and Democrats were essentially committed to the same goals, for all the differences in their style and tactics, Watson finds their bitter disputation reflective of their drastically dissimilar values and objectives. Watson's title is not a mere rhé-



torical device but a token rather of his belief that the conflict between liberty and power discerned by John Trenchard and Thomas Gordon in eighteenth-century England was also at the center of American politics during the second quarter of the nineteenth century. According to Watson, "liberty and power were indeed at issue" in the crucial presidential election of 1828 and "President Jackson took office as the friend to liberty and the foe of unwarranted power" (pp. 93, 95). There is a certain novelty and subtlety, too, in this reading of an election that many contemporaries and later historians alike regarded as one of the dirtiest, most issueless, and least principled in American history. Subtlety may enter into Watson's choice of the modifier "unwarranted" as his perhaps indirect way of suggesting that a man so dedicated to increasing his own power as was Jackson felt that a like accrual of power by his many critics was unjustified.

In view of the abundance of scholarly studies that have been published since 1959 and the strikingly new themes, approaches, and methodologies characteristic of this literature, it is hardly surprising that a historian who has absorbed it as conscientiously as has Watson would treat and appraise events and issues in ways undreamed of by Van Deusen's scholarly generation.

The strength of a book depends of course not on whether its author subscribes to one rather than another interpretation but on its intellectual quality, whatever its viewpoint. Our concern is with the good sense of its appraisals, the justice they do to the evidence and the fairness with which the evidence is read, the originality and insightfulness of its arguments and the persuasiveness of these arguments. A strikingly original interpretation that strains logic and misconstrues evidence does not impress us. By this standard, Watson's volume is wanting.

There is occasional bias. In contrast to the Jacksonians, their opponents "snort" and their editorials critical of Jackson's actions "shriek." Adams's unwillingness to fire able civil servants because of their political preferences manifests his "dogmatic indifference to [the] practical politics" pursued by Democratic spoilsmen (p. 86). But President Jackson's readiness to fire competent officeholders and replace them with men "more in agreement" with his politics was designed to "avoid the creation of an entrenched and corrupt bureaucracy" (p. 103). Fortunately, such examples of partiality are few and, I believe, unwitting. Although he clearly has much greater admiration and sympathy for Jackson and his party than for their adversaries, Watson does not blink from reporting the Jacksonians' deficiencies, demonstrating an exemplary fairmindedness and integrity. The problem is that his readings of much of the evidence are unpersuasive, he often seems oblivious to outrages perpetrated by the politicians he favors, and he treats one individual's comments as representative of the thinking of a group. In rightly rejecting the view that

the public rhetoric of realistic major party politicians is invariably claptrap, Watson often takes the mouthings of office-seekers at close to face value, notwithstanding the absence in their careers of any suggestion that they lived up to or acted on the lofty principles they proclaimed to the electorate.

Portraying materialistic, proslavery Democratic leaders active in land and financial speculation as a species of libertarian democratic socialists dedicated to an egalitarian commonwealth, as Watson does, is imaginative but not persuasively so. Appraising party leaders cheerfully tolerant of, where they did not practice, corruption as fervent devotees of the "virtue" that an ingenious modern school of historical thought assures us was believed necessary for a republic, is puzzling, as is the suggestion that the Jacksonians' hostility toward blacks and Native Americans was a "logical" byproduct of their public "insistence on white male equality" (pp. 242-43). If Democrats rejected people of color because they championed the rights of whites, Whigs it seems were "more willing than Democrats to accord partial rights to blacks and Indians" because of Whig "acceptance of human inequality" (pp. 215, 246). There may be a consistency of sorts in such appraisals but it is of the kind that induces wonder about hobgoblins.

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DOROTHY STERLING. *Ahead of Her Time: Abby Kelley and the Politics of Antislavery*. New York: W. W. Norton. 1991. Pp. 436. \$22.95.

Abby Kelley was a pioneering abolitionist and feminist whose election to the executive committee of the American Anti-Slavery Society in 1840 precipitated a major rift within antislavery ranks and made her the target of misogynist attacks. Indeed, the term "Abby Kelleyism" became almost the equivalent of "Fanny Wrightism" as an epithet for shameless women. In this first full-scale biography of Kelley, Dorothy Sterling brings together materials from widely scattered sources in order to tell Kelley's life story, and to secure for Kelley a place in the galaxy of American heroines.

Sterling makes clear Kelley's importance to the history of abolitionism, especially to its Garrisonian wing. Having grown up as a Quaker farm child in Worcester, Massachusetts, Kelley developed a strong interest in antislavery and nonresistance while a teacher in Lynn, then formed a conviction that she had received a "divine call" (p. 71) to lecture on the evils of slavery. Her commitment never wavered. Neither her marriage to Stephen S. Foster in 1845 nor the birth of their daughter Alla in 1847 nor even the intense hostility that her work evoked diverted her from lecturing, fund raising, and organizing. Among her notable successes was recruiting into the ranks of activists talented women such as Lucy Stone

and Paulina Wright Davis, and her ability to raise money when even the deepest financial wells seemed to have run dry.

Sterling is particularly adept at conveying the aura of Kelley's milieu. One comes to know the world of "ultraist" reformers like Stephen Foster and Abby Kelley Foster, who depended heavily on networks of like-minded friends and associates to sustain them as they pursued difficult and unpopular work, but who often disagreed vehemently with friends as to strategy and tactics. At various times, both Kelley and Foster (described even by fellow workers as a fanatic) came into conflict with William Lloyd Garrison, Frederick Douglass, Elizabeth Cady Stanton, and Susan B. Anthony. "Advancing age always tends to conservatism," Kelley remarked (at age 51) during one such wrangle; "we should pray to be preserved in the freshness of our fanaticism" (p. 335). One also gains a fresh appreciation for the power of nineteenth-century gender hierarchy and gender conventions. Denounced as a "modern Amazon" (p. 194), a "man woman," or a "nigger bitch" (pp. 13–14), and physically threatened by antiabolitionists, Kelley had to contend as well with abolitionists who blamed her for the split in 1840 and slandered her as a modern-day Eve, Delilah, or Helen of Troy, and as a devotee of "PETTICOAT GOVERNMENT" (p. 108).

In her desire to rescue Kelley from historical neglect, Sterling at times errs in the direction of hagiography and internal perspective. We view Kelley and her ideas almost exclusively from her own standpoint, or from that of her cohorts, who represented the most radical wing of a small and generally despised social movement. As a result, abolitionist (and feminist) disagreements on strategy and tactics sometimes lose their complexity. Nor does Sterling take full advantage of the opportunity to analyze Kelley's motives or drive. Although this is a life-and-times biography, and not a psychological study, some analysis of the type that Robert Abzug brought to his biography of Theodore Dwight Weld (*Passionate Liberator* [1980]) would have been welcome. Indeed, historians of reform will wish that the book addressed historiography more directly. Sterling's discussion of "come-outerism" would have benefited from a look at John McKivigan's *The War Against Proslavery Religion* (1984), and her analysis of divisions within the Boston Female Anti-Slavery Society could have been informed by Debra Gold Hansen's work ("Bluestockings and Bluenoses: Gender, Class, and Conflict in the Boston Female Anti-Slavery Society, 1833–1840," Ph.D. diss., 1988). As Hansen showed, it was social class, not race, that proved a key agent of fissure. But overall this is an interesting, well-researched, and engaging book that adds luster to the career of both its subject and its author.

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THOMAS P. SLAUGHTER. *Bloody Dawn: The Christiana Riot and Racial Violence in the Antebellum North*. New York: Oxford University Press. 1991. Pp. xiv, 252. \$29.95.

On September 11, 1851, in the town of Christiana, Pennsylvania, a crowd of black women and men resisted the efforts of a Maryland slaveowner to reenslave four men who had fled from his plantation to free soil two years earlier. In the ensuing riot, the slaveowner, Edward Gorsuch, was clubbed and shot to death and his body mutilated, after a confrontation with Samuel Thompson, one of the fugitives from Gorsuch's farm. The riot was followed by a reign of violence and terror conducted by white posses against local blacks and the arrest of thirty-eight men on 117 separate counts of treason, the largest mass indictment for that crime in American history. Gorsuch had come to Christiana armed with the power of the new Fugitive Slave Act, and the trial for treason became a national cause célèbre—a test case for the controversial new law and an event that clarified sectional commitments and inflamed the tendencies to violence already manifest in antebellum American culture.

Thomas P. Slaughter's book goes well beyond Jonathan Katz's *Resistance at Christiana* (1974), a still useful "documentary account," to use the riot as a means of exploring the master-slave relationship, the significance of freedom for free blacks, and the nature of relations between black and white Northerners before the Civil War. Gorsuch was a devout Methodist who perceived himself as a benevolent master and who arranged to liberate his twelve slaves when they reached the age of twenty-eight. The four men who escaped his farm had been caught stealing grain for resale, and Gorsuch hunted for the men out of feelings of injured honor more than a material need to recover his lost human property. Slaughter is adept at reconstructing the motivations of the slaves and the master, and the psychology of their relationship, showing how Gorsuch was dependent on the fugitives for his own sense of identity, and how violence was implicit in slavery and became a part of the fugitives' quest for freedom and a break with the past. He makes a further contribution with his imaginative and deeply probing readings of late-eighteenth and nineteenth-century Lancaster County newspapers and court records, which allow him to evaluate the "justice" of the Christiana rioters' trial in the context of fine-grained judgments about changing racial attitudes and the relative status of different sorts of social and cultural "outsiders" before the legal system and in the community. Slaughter illuminates a domain of black poverty and race relations in the antebellum rural North of which we heretofore knew little, and, like Nat Brandt (*The Town That Started the Civil War* [1990]), he demonstrates the central, animating role that popular black activism played in the culture and politics of the pre-Civil War North.

It should be noted that a final summary—relating the excursion into local court records back to the Christiana riot and, more broadly, to what the title refers to as “racial violence in the antebellum North”—is not satisfyingly made. Slaughter asks (p. 180): When “racial outsiders murdered a territorial outsider, how would the community react?” His answer is far too brief. Two contexts might have been further delineated and explored, those of antebellum northern black activism and millennial Protestantism. The second of these (we know from Katz’s narrative) was the idiom in which the fugitives addressed the deeply religious slaveowner Gorsuch at dawn on the day of the riot, and was a mental framework through which many mid-nineteenth-century Americans comprehended and justified violence. But these observations hardly detract from this compelling and timely book.

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MELTON A. MCLAURIN. *Celia: A Slave*. Athens: University of Georgia Press. 1991. Pp. xi, 148. \$19.95.

In November 1855, “Bloody Kansas” prefigured the bloodshed of civil war; in nearby Callaway County, Missouri, the trial of Celia, a slave, reflected the oppressiveness of slavery. Melton A. McLaurin effectively relates these events to each other and convincingly argues that Celia’s case exemplifies the fundamental moral horror of bondage.

Robert Newsom, a sixty-year-old widower, bought fourteen-year-old Celia, raped her soon after the purchase, and continued to do so thereafter. Celia, as a slave, had no legal recourse. George, another Newsom slave and Celia’s lover, insisted that she terminate the degrading relationship. Celia planned to do so the next time Newsom tried to force her. Knowing he might react violently, Celia kept a large stick nearby. She struck Newsom on the head when he forcefully demanded sex; she dealt him another, fatal blow when he continued to advance toward her.

Powerless to protect Celia or himself, George implicated her. At first resolute in her denial of guilt, Celia bowed to the overwhelming coercive power of her questioners. The Callaway court heard the trial, the jury found Celia guilty; an apparent rescue of Celia from prison allowed time for a fruitless appeal to the Missouri Supreme Court, and Celia was executed.

McLaurin faces the problem that only a few sources concerning enslaved African Americans contain their testimony. Relying heavily on trial testimony by whites, he tries to explore the significance of these two violent deaths. He attempts to compensate for the one-sided sources by examining the circumstances in which Celia found herself.

Using recent studies of enslaved women as well as of the legal disabilities of enslaved defendants in criminal trials, McLaurin finds implications in Celia’s case for relationships between enslaved African-American women and African-American men, white men, and white women. Unable to defend Celia, George’s only option was self-preservation. The slave codes and white men regarded enslaved women as insignificant and therefore without any effective defense against someone like Robert Newsom. White male domination of white females assured the failure of Celia’s petition to Newsom’s daughters to dissuade him from further assaults.

McLaurin’s conclusions are open to argument. He takes seriously a court defense that was not a defense and a rescue that was ultimately not a rescue. It is difficult to see as “audacious,” “bold and imaginative,” “brilliant,” or “innovative and daring” (pp. 89–91) a defense that Celia’s lawyers knew had absolutely no chance of succeeding. McLaurin also overlooks the fact that, if George fathered the baby Celia was carrying, he and Celia had a moral claim to the baby that was superior in their eyes to Newsom’s legal claim. The two slaves’ claim could have forced the issue as much as or more than did George and Celia’s relationship. Finally, it is particularly difficult to identify slaves’ moral decisions concerning the desperate dilemmas slavery created for them. Could it not be that George, who regarded his relationship with Celia as licit and Newsom’s as illicit, nevertheless rejected Celia’s killing Newsom as immoral? Because such questions deserve attention, this book merits both a wide readership and extensive discussion.

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RICHARD H. ABBOTT. *Cotton and Capital: Boston Businessmen and Antislavery Reform, 1854–1868*. Amherst: University of Massachusetts Press. 1991. Pp. x, 294. \$32.50.

This intriguing study by Richard H. Abbott details the extensive political initiatives undertaken by a cluster of mid-nineteenth-century Boston businessmen who rejected the quiet complicity with slavery common among those with southern trade ties as well as William Lloyd Garrison’s militant abolitionism. Seeking a middle path toward promoting freedom for African Americans, textile millmen Amos Lawrence and Edward Atkinson, merchant and railroad magnate John Murray Forbes, and several dozen wealthy associates exemplified the American rage for association by starting and funding a remarkable array of activist institutions that spanned the years from bleeding Kansas to the Freedmen’s Bureau. Throughout, as Abbott painstakingly documents, “they sought to mobilize white self-interest, not disinterested humanitarianism” (p. 5), to limit,

then eradicate, slavery, and to repair the damage it inflicted on the millions held in bondage. Yet the frustrations, failures, and unanticipated consequences of their efforts are an ironic counterpoint to these exercises in practical reason, hinting at the limitations of interest-based liberalism, an issue familiar to students of modern America.

The antislavery moderates' pivotal institution was the New England Emigrant Aid Company (1854–69), which promoted the relocation of Yankee farmers to Kansas through propaganda, travel grants, and land purchases. Although it assisted some 3,000 migrants and founded durable towns (Lawrence, Topeka, Manhattan), the company could gather neither sufficient funds nor adequate momentum to make a substantial impact. More troubling, once fighting broke out, it bought and shipped westward hundreds of weapons, its directors quietly funded John Brown's expenses, and a parallel emergency committee forwarded Brown 200 additional Sharp's rifles in 1857, guns that were ultimately used in the 1859 Harper's Ferry assault. Divided in the party politics melees after 1856, Boston business activists united again in wartime fundraising for local and border state regiments, the Sanitary Commission, and a league to circulate antislavery pamphlets and packaged articles for unionist newspapers. Repeatedly, as Abbot notes, instead of "stressing emotional appeals," they relied on "practical, dollars-and-cents concerns" to argue the economic benefits of emancipation and the political necessity of prosecuting the war (p. 75).

This approach continued into their disastrous recruitment of immigrants for Union armies and a successful drive to legitimize African-American regiments, both intended to "keep the working classes . . . manning the factories on which the state's prosperity, and theirs, depended" (p. 117). This awkward mixing of public and private interests also haunted their engagements with the freedmen's concerns for land, capital, and political power into the 1870s. Overall, although Abbott's choice to refrain from commenting on his subjects' then-typical racist language is perhaps defensible, it is disappointing that he does not conclude by reassessing the cited, recent literature on practical philanthropy or the politics of war and reconstruction, and is indifferent to the theoretical issues this narrative raises about both voluntarism and institutions intermediate between individual and state or society. While one cannot but admire the relentless energy of these activists, Atkinson's claim to Forbes in 1865 that "the dollar argument would succeed where the argument on abstract rights would fail" (p. 168) remains as problematic now as over a century ago.

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RALPH E. LUKER. *The Social Gospel in Black and White: American Racial Reform, 1885–1912*. (Studies in Religion.) Chapel Hill: University of North Carolina Press. 1991. Pp. xiv, 445. \$39.95.

The judgment has long prevailed that the advocates of the Protestant Social Gospel ignored the issue of race. With the publication of Ralph E. Luker's important book, based on extensive archival research, that judgment can no longer stand.

Luker locates the origins of the Social Gospel not in the late-nineteenth-century reaction to industrial crisis but in antebellum home missions and voluntary societies. He shows that in the first two decades after the Civil War, reformers extending the work of these prewar institutions sought to elevate former slaves through education, debated the virtues of selective colonization in a redeemed Africa, and founded new organizations such as the National Citizens' Rights Association and the Afro-American League to promote the civil rights of African Americans.

In the 1890s all three efforts foundered. The schools of the home missions movement suffered from financial reverses, the colonization movement proved itself morally bankrupt, and the southern denial of black voting rights left reformers arguing among themselves about disfranchisement. Most could do little more than protest the spread of lynching. But a minority, black and white, regrouped behind programs for education, settlement houses, and institutional churches, and many of them struggled against debt peonage, the convict lease system, and the denial of civil rights.

On issues of race, the Social Gospel movement as Luker presents it was a joint effort between white clergy and reformers, such as Washington Gladden and Robert Ogden, and black leaders like Ida Wells, Booker T. Washington, and Reverdy Ransom. It was by no means unified. It covered a spectrum of views on every pivotal question, and it embraced social philosophies that ranged from the theological progressivism of Harlan Paul Douglas, the cultural assimilationism of Josiah Strong, and the humane comprehensiveness of Josiah Royce, to the conservative separatism of Edgar Gardner Murphy and the radical racist separatism of Thomas Dixon, Jr. Luker believes that its overarching direction on racial as well as economic questions was toward conservatism in the sense that its proponents appealed to social and religious values embedded in earlier American traditions.

Luker makes a good case for broadening the definition of the Social Gospel to include the white and African-American reformers who struggled for solutions to racial strife and injustice. But it is not clear why he includes the ideas of Dixon, a radical white racist, within the "spectrum of white social gospel thought" (p. 289). He believes that substantive definitions of the movement have been arbitrary and selective, resulting in the exclusion of race as a Social



Gospel issue, and he leans toward a formal definition of it as a conservative wariness of changes wrought by industrial capitalism. By that criterion, Dixon fit into the movement, but the implication of including him is that the "Social Gospel" consisted of everyone who, adducing religious warrants, took any public position on questions of economics, politics, or race. The question of definition is not fully resolved.

Nonetheless, Luker has made it impossible for us to rest content with earlier assessments of the Social Gospel. He has written a superb revision of older assumptions, marked by solid research, sensible judgments, and moral wisdom.

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SUSAN CURTIS. *A Consuming Faith: The Social Gospel and Modern American Culture*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1991. Pp. xviii, 320. \$38.85.

Despite the acknowledged importance of the Social Gospel as an initiative among late-nineteenth-century liberal Protestants to remedy the problems of urban industrial society, scholars have been divided about the movement's historical boundaries and meaning. Sydney Ahlstrom, for one, observed in *A Religious History of the American People* (1972) that "when the Social Gospel movement began and when it came to a close are much disputed questions." In terms of social location, the Social Gospel's proponents have been placed precariously between Protestant liberalism and secular progressivism, either as reform-minded Christians or religiously oriented advocates of social reconstruction. Most crucially, historians' interpretations of the motives that impelled the activists have alternatively emphasized status seeking, social control, and dedication to a set of religious values.

Susan Curtis's book is the first major study of the Social Gospel in a number of years and takes a coherent stand on these issues. She argues that the Social Gospel belongs in time between Victorian evangelicalism, with its focus on individual salvation, and the mass-oriented consumer culture that achieved dominance in the 1920s. Indeed, Curtis explains that the Social Gospelers' search for effectiveness led them to an affinity with such standards of the marketplace as popularity and simplicity of presentation, a situation that made the Social Gospel, in one of several senses, "a consuming faith" and ultimately jeopardized its critical power. With respect to who the Social Gospelers were, Curtis stresses their Protestant, as opposed to Progressive, ties. Her analysis centers on sixteen biographies of men and women who were ministers, seminary professors, and denominational organizers and who insisted that Christianity had a social mission—the intellectual core of the Social Gospel—as a result of their rejection of the Victorian

Protestantism of their childhoods. Curtis enters the debate on the sources of the Social Gospel by insisting that its moving force was personal crisis, the need of its participants to formulate "a religious ideology that provided comfort and meaning to them" (p. xii). She is more concerned with individual decisions than with reform programs and, because of this interest, examines the implications of the Social Gospel's religious commitments for work, family life, and political involvement.

The book's principal strength is its author's success in humanizing the liberal Protestants who gave voice to the Social Gospel. This accomplishment entails more than writing biographical sketches that are sensitive to private conflicts. Curtis takes religious convictions seriously as key components of personal orientation and as potent influences in society. The dramatic shifts in class relations at the turn of the twentieth century have tempted historians to highlight the self-seeking social agendas of middle-class reformers and to slight the psychological importance of their Christian terms of expression. Most likely, the motives of the Social Gospelers were mixed, but Curtis's appreciation of religion's role in this period helps to restore balance to scholarly discussion. In addition, Curtis's insistence that religious beliefs are part of culture, not simply an extrinsic force, spurs her often provocative investigations of connections between convictions and ostensibly secular activities.

The book is less satisfying on methodological grounds. Curtis would have better prepared her readers to evaluate the reliability of her conclusions had she more fully explained the logic of her approach. It is not clear why the individuals she chose are illustrative of the Social Gospel or why writing a series of biographies is a revealing analytic strategy. In addition, in at least one serious instance she uses sources without sufficient skepticism. She does not question the description of Victorian individualism found in the Social Gospelers' writings and thus misses the strong social expressions, even if based intellectually on an insistence on personal conversion, of earlier Protestantism. Abolitionism, temperance, and missions were just some of the reform movements contemporaneous with the Social Gospelers' childhoods (Curtis's subjects were born between 1835 and 1878). Curtis might have wondered whether the advocates of the Social Gospel were in fact heirs to the Protestant tradition as well as rebels against it.

Curtis has achieved an intelligent and engaging view of a seminal American Protestant religious movement. She has not provided definitive answers about the Social Gospel, but, perhaps more important, she will stimulate informed discussion by the valuable assumptions and generally well-executed research of her study.

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LOUISE L. STEVENSON. *The Victorian Homefront: American Thought and Culture, 1860–1880*. (Twayne's American Thought and Culture Series.) New York: Twayne. 1991. Pp. xxvii, 235. Cloth \$28.95, paper \$12.95.

In her survey of American thought and culture between 1860 and 1880, Louise L. Stevenson concentrates on one group of Americans, the Victorians, whom she identifies as men and women of the middle class. Hers is no anthropological approach—no rituals of public holidays, asserts Stevenson—nor the approach traditionally assumed by intellectual historians, a study of great men and their great ideas. Instead, Stevenson elects to consider the manner in which Victorian beliefs and values were disseminated and received. She begins with the institutions of Victorian intellectual life, first the home, and then, moving outward, cultural and educational institutions. Stevenson next turns to the spokespersons of Victorian intellectual life, the key terms and issues of their discourse, and the variety of stances they assumed within that discourse.

By rethinking the concept of intellectual history to include not just card-carrying intellectuals but instead all those who participated in intellectual life, Stevenson is able to consider categories of Americans—women, African Americans, southerners—types of institutions—the Victorian parlor, the women's reading club—and kinds of evidence—parlor furniture, schoolbooks, best-selling novels—often excluded from intellectual histories. But while Stevenson strives to show the different meanings that the Victorian world view held for men and women, blacks and whites, northerners and southerners, she underplays some critical divisions within that intellectual world, those relating to religion and class. What separated evangelical, middlebrow Victorians from those of the secular and highbrow variety is at least as important as what they held in common. Stevenson does indeed refer to these differences, often with unusual insight—when contrasting the parlor organ with the parlor piano, when comparing Dwight Moody and Phillips Brooks, when distinguishing between “emotional” and “intellectual” Victorians (p. 27), or, again, between “democratic” and “republican” intellectuals (p. 180)—but these references can be found scattered in as many chapters. Other dichotomies are treated just as suggestively, but also just as intermittently and hence ineffectively, namely those between the ideals of community and individualism, between democracy and elitism, and between a universe ruled by God and that explained by modern science. Just how these sets of tensions are related thus remains frustratingly obscure.

Much of the problem is organizational. The content of many chapters seems random, skewed, or inappropriately dictated by the category of research material. Also problematic is Stevenson's treatment of the relationships among culture, class, and power.

While admitting, for example, that such new urban institutions as museums and parks “were not entirely responsive to public tastes” and were in fact purposefully “insulated . . . from the influence of mass behavior” (p. 63), Stevenson insists that they nevertheless “represented the public's aspirations and beliefs” (p. 60). She characterizes the goal of disciplining and enlightening patrons as ultimately democratic, even as she presents evidence—the Sunday closure of the Metropolitan Museum of Art for its first generation of existence, the outlawing of rough sports in parks—that points to the cultural elite's desire to differentiate itself from and indeed control the masses. The author is under no obligation to treat any but the Victorian middle class—that is her self-defined task—but she must then address the class functions of Victorianism.

Thus, what one looks for in vain are cohesive, extended discussions of the tensions within Victorian culture that in fact define the Victorian intellectual world. Had these tensions assumed their rightfully central role, the reader would have gained a much better sense of the strains under which the Victorian consensus was due to shatter.

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GEORGE COTKIN. *Reluctant Modernism: American Thought and Culture, 1880–1900*. (Twayne's American Thought and Culture Series.) New York: Twayne. 1992. Pp. xvi, 188. Cloth \$28.95, paper \$12.95.

Those of us who see Modernism as the new world view of ideas and values that replaced Romanticism/Victorianism in the early twentieth century will not agree with George Cotkin's usage of the word in this book.

Although Cotkin uses the word “modernism” in his title, he never satisfactorily defines it, except as “an elusive but compelling conceptualization,” then as “an adversarial culture” (p. xii). The word used most often in the book in reference to the “modern” is “modernity,” which he states “may capture everything from the Cartesian philosophical revolution of the seventeenth century to the momentous change in the late nineteenth century that ushered in a bureaucratized and rationalized vision of the world” (p. xii). He identifies as reluctant Modernists those he perceives as trying to find a middle ground between Victorian ideals and “the precipice of modernity” (p. 153). Those who see the modern primarily in these terms may find no problem with Cotkin's interpretation of cultural and intellectual trends in the last twenty years of the nineteenth century in America.

But for those of us who define Modernism in the first way, Cotkin's interpretation is not satisfactory. Thinking in these terms, the title implies that the period from 1880 to 1900 was already a time when a new world view, Modernism, was raising important

questions in American cultural and intellectual life. That was not so. The era was firmly based in nineteenth-century ideas, key among them the belief in Progress, in the orderly development of humanity, and the organic nature of the universe as it grew and developed, that were unquestioned by the majority of thinkers and writers. Ideas such as Charles Darwin's were absorbed into this world view prior to 1900. Modernism begins with the questioning of this world view, and it is with the abandonment of the idea of linear progress and the acceptance of relativity as a key concept that Modernism comes of age. Moderns also begin to accept and deal with the idea of chance in the evolving universe, rather than ignoring it. With this understanding, most of the intellectuals and cultural figures of the period 1880–1900 were not reluctant Modernists but rather questioning Romantic/Victorians, those who placed the new ideas neatly within the old framework and lopped off those parts that did not fit, as Cotkin himself demonstrates. Some, like William James, certainly were feeling their way toward the new Modern world view, but most of the theologians, most of the major anthropologists of the period, and several of the philosophers Cotkin discusses were not.

While Cotkin's interpretation of the concept of the modern is debatable, his scholarship is excellent. The purpose of his book, as part of the Twayne American Thought and Culture Series, is to survey the prominent intellectual and cultural issues in America from 1880 to 1900. In stylish, readable prose Cotkin explains various intellectual and cultural currents, including the debate over evolution in religion, the movement toward a psychology distinct from philosophy and a philosophy based on experience and process, the effect of Darwin's ideas on anthropology and its acceptance of racism, women's intellectual and cultural issues, the attempts of cultural custodians to influence the masses and the parallel beginnings of consumer culture, and *fin-de-siècle* themes in architecture, literature, and art.

Each chapter gives a brief summary of major figures in these various areas, their important writings, and their significant arguments on the different sides of these developments of the last twenty years of the nineteenth century. Because of its clear organization and content, this is a book that will be useful to undergraduates as well as others looking for a quick overview of major ideas and cultural events of the period, although the interpretation of the time as one of reluctant Modernism could use some clarification.

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ROBERT DAWIDOFF. *The Genteel Tradition and the Sacred Rage: High Culture vs. Democracy in Adams, James, and Santayana*. (Cultural Studies of the United States.) Chapel Hill: University of North Carolina Press. 1992. Pp. xxiii, 218. \$32.50.

This is a delightful and witty book, brimming with moral purpose and strong interpretations. Robert Dawidoff wants to demonstrate how Tocquevillian detachment, disdain for democracy, and intellectual elitism have become the elements of an entrenched and ultimately self-defeating style of American cultural criticism. Tocquevillian strains are found to be central in Henry Adams's deep alienation from American democracy and in the condescension that permeates George Santayana's famous essay "The Genteel Tradition in American Philosophy" (1911). A chapter evaluating Henry James's *The Ambassadors* (1903) charts James's distanced but typically nuanced view of American culture.

Dawidoff opens with a shaky premise: that Tocqueville must be understood as distant from and antagonistic to American culture. While his analysis of American democracy was marked by much ambivalence and antagonism, Tocqueville also respected the benefits of democratic equality in America. Moreover, in considering the life of the mind, Tocqueville noted that intellectual revolutions might occur best in a society that neatly balanced equality and aristocracy: such a system might develop in America, albeit with a natural rather than an entrenched aristocracy. In sum, Tocqueville was neither as strong a traditionalist nor persistent an antimodernist as Dawidoff suggests.<sup>9</sup>

However uncertain Dawidoff's initial use of a Tocquevillian straw man may be, it does not topple or diminish the wisdom and originality of his individual interpretations of Adams, James, and Santayana. All of these thinkers found the American cultural and intellectual landscape boring, stripped bare by the dangerous leveling effects of democratic equality and by the pallid moral evasions of the "Genteel Tradition." They worried, in Dawidoff's view, about how "high culture" and deep thought could possibly thrive in American culture. Burdened by such worries, James and Santayana became expatriates, while Adams went into a kind of internal exile.

The strongest chapter in the book is a sustained analysis of James's *The Ambassadors*. In a close reading of the text, Dawidoff finds examples of the shift from "the sacred rage" (Puritan moralism) to the genteel tradition (the idealization of culture). Although freed from the powerful moralism of the Puritan tradition, many characters in the novel remain tied to the beliefs and illusions of genteel culture that glossed over the industrial and sexual realities of late-nineteenth-century America. Another strong chapter comprehends Santayana through a conceptual framework of marginalization. Santayana functioned out of a repressive culture of the closet that America demanded for those who were homosexual. Closet culture helped to make Santayana's observations of American culture particularly cutting and observant, but his accompanying detachment ultimately fostered deep elitism, alienation, and distaste for democratic possibilities.

This is a book to be savored as much for its jauntiness (as when Dawidoff compares James's novelistic approach with the genre of detective fiction) as for its sustained interpretations (as when Dawidoff argues that the ghost of William James stalks the text of "The Genteel Tradition in American Philosophy"). Dawidoff has not written a work of traditional history, marked by a demanding contextualism or distanced authorial point of view. Nor, unfortunately, will readers find any extended grappling with the work of other historians with relevance to Dawidoff's topic, such as Lawrence Levine and T. J. Jackson Lears. Instead, Dawidoff takes his readers on a rewarding ramble through the thickets of cultural criticism in which an insightful author struggles with, while also appreciating, his brilliant subjects. Yet all historians will benefit if they approach this book in the same spirit that Dawidoff suggests that citizens should confront American culture today: by placing yourself within the world around you.

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JOAN SHELLEY RUBIN. *The Making of Middlebrow Culture*. Chapel Hill: University of North Carolina Press. 1992. Pp. xx, 416. Cloth \$34.95, paper \$14.95.

When in 1891 Joseph Simms set down the physiological means for distinguishing between "superior" and "inferior" types, he linked culture, race attributes, and the look of brows. Alphonse de Lamartine, represented by a forehead with a vertical sweep, is paired with an Irishwoman whose ape-like brow denotes the cultural bog out of which her race has barely emerged. The lofty brow of a white boy is contrasted with that of a black boy fated to "feeble intellect." One of Simms's pairings, however, stands apart from his overdetermined highbrow/lowbrow oppositions. Next to an Indian—"as witless as a dry stump"—are the familiar features of Mark Twain. But what does this famous physiognomy actually signify? Does Twain's extravagant brush of hair conceal a middling brow? Where precisely does he stand in regard to a national culture in which genteel tradition and popular humor seem at odds?

In 1920 Van Wyck Brooks would tease at the questions arising from Twain's uneasy relation to the high culture, but it is Brooks's seminal essay of 1915, setting "highbrow" in contention against "lowbrow," that first expressed his doubts as to the kind of culture Americans possess. As Joan Shelley Rubin's admirable study makes clear, Brooks's declared regret that the United States lacked a "genial middle ground" prompted Dwight MacDonald and Clement Greenberg, for example, to deplore the "Midcult" whose attempts at "mediation" means "mediocrity" (pp. xiv, xix). Fortunately, Rubin contributes light in place of the heat, smoke, and mirrors that too often

obscures what is involved in the making of a middlebrow culture.

Rubin concentrates on five main areas of middlebrow culture and the intricate processes that went into their making in the years between 1917 and 1950: the guidance offered by the *New York Herald Tribune Books* section under the wing of the New Humanists; the activities of the original selecting committee of the Book-of-the-Month Club; the emergence of the "great books" plan; the influence exerted by the masters of "outline" histories; the rise of book commentary on commercial radio and the collapse of the quiz show phenomenon.

Rubin opens with an overview of the nineteenth century in order to validate her argument that the formation of the middlebrow "self" was always charged by cultural imprintings. This material, however useful, is somewhat dogged in its presentation; it is informative in ways rather too reminiscent of the Chautauqua lectures and Dr. Eliot's Five-Foot Shelf with which she deals. Chapter 2 is also more edifying than engaging in its account of how "High Journalism" rose from the merger of the genteel tradition with the concerns of the New Humanists. But if Rubin cannot quite make these chapters work, she does so, wonderfully well, in those that follow.

Through a series of biographies, Rubin traces childhood influences, college training, and professional concerns that made the culture makers what they were. She revivifies old controversies between the specialist and the generalist, the possession of culture and the attainment of information, the authority of elites and the public's autonomy of choice. She makes it matter that we learn how the Book-of-the-Month Club selections were made; how "merchants of light" like Will Durant turned the historical panorama into middlebrow art; how the "schools of the air" in the 1930s led to the "Twenty-One" debacle of 1957; how the "Swift Hour" interspersed tips on good reading from "beloved" "Billy" Phelps of Yale with guidance in "the selection of fine foods for your table" (pp. 283–84).

Rubin provides a thorough, and often thoroughly absorbing, sociocultural history of those who eagerly drew from diverse materials appropriated from above and below in their quest for "voluntary education" (p. xii). Middlebrows were manipulated, to be sure, induced to believe that they chose their culture as members of the right "club" or "school," but Rubin maintains they were served more than they were harmed. She arrives at this conclusion despite her apprehensions over the "unresolved tensions and contradictions" that reflected "a society in transition" and over her own "middleness" (p. xx). (After weathering his night among the Brahmins at the infamous Whittier Birthday Dinner in 1877, Mark Twain would have agreed, filled as he was with that same middleness.) More sophisticated assessments than Rubin's may yet be written, but in their highbrow



approach, future analysts might lack the virtues exemplified by her noteworthy study.

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MICHAEL KAMMEN. *Mystic Chords of Memory: The Transformation of Tradition in American Culture*. New York: Alfred A. Knopf. 1991. Pp. viii, 864. \$40.00.

Michael Kammen's undertaking in this mammoth work of research and synthesis is to explore the nexus between "collective memory" and "national identity" in the United States. The scope of the book and its erudition are awesome, its subject virtually boundless, and the approach holistic: an effort, epic-like in scale, to disclose what functions "tradition," and also its opposite, amnesia, have played and still play in American culture. Kammen is concerned with the motives that have shaped various reconstructions of the past (selective remembering and forgetting) in American life from 1870 to the present, but the book so richly records the resonances of American chords of memory that its foremost impression is that of an encyclopedic account of changing versions of the past in American public and everyday life.

Employing a convenient periodization that corresponds to major movements in world history (1870 to 1915, 1915 to 1945, and 1945 to 1990), Kammen provides astonishingly detailed evidence of diversity in American forms of historical consciousness. The data include private collections of artifacts, public commemorations of local heroes and historical events, restoration and preservation of historical homes and places, the establishment of organizations (the American Historical Association, for example) responsible for "history," public monuments and historical paintings, fiction, memoirs, personal correspondence, advertisements, world's fairs, popular prints, magazine stories, and newspaper editorials. In Kammen's egalitarian culturist enterprise no material is too ephemeral or slight to find a place in this compendium of evidence of a national habit, the reconstruction or invention of versions of the past to suit particular needs and desires.

The book sets out to give an account of American uniqueness but intelligently avoids a parochialist slant. A valuable international perspective appears in a "coda" following each of the three main divisions of the book. American distinctiveness in the uses of history for nationalist purposes lies in the nation's pluralism. Mindful of cultural diversity, Kammen takes scrupulous account of regional, racial, sexual, and ethnic differences. He weaves contests of meaning and value, regarding the Civil War, for example, or the place of Native Americans within a national consensus of "our" history, into his account of the fabric of national identity. A cast of hundreds includes significant minorities, making this perhaps the

most representative work in the historiography of American culture. The author's aim is to comprehend a whole whose parts are often at odds with each other, yet remain parts of the same whole.

The heart of the book is its concern with the nationalist dimension of American culture. Kammen asks (p. 7), "[w]hen and how did the United States become a land of the past, a culture with a discernable memory (or with a configuration of recognized pasts)?" Tracing how and when various memories emerge as recognizable features of the national culture, Kammen employs concepts familiar to students of American culture, such as the persisting quarrel between the party of hope and the party of memory, between modernists and traditionalists—often embodied in conflicts between populists and elitists. While "conflict and complexity" are keynotes of the "transformation" in the uses of history since 1870, and a "politics of culture" is the principal mode of this transformation, the author argues for an underlying coherence in the story.

Although the argument lies loosely on the extraordinary flow of detailed examples, the book moves steadily toward a conclusion that minimizes conflict over consensus. Kammen states the conclusion at the outset: "Although there have been a great many political conflicts concerning American traditions, ultimately there is a powerful tendency in the United States to depoliticize traditions for the sake of 'reconciliations.' Consequently the politics of culture in this country has everything to do with the process of contestation *and* with the subsequent quest for reconciliation" (p. 13).

In arguing for "consensus and stability" (p. 14), Kammen does not slight evidence of dissent and contest. Among its many virtues the book provides excellent accounts of the nexus between nostalgia and modernism in the early twentieth century, of conflicts of interpretation in the folklife revival, and of the role of entrepreneurship in the rapport between "American heritage" projects and commercial tourism in the Cold War period. Consensus and stability may remain contested views of American reality, less the self-evident propositions that the author assumes, and indeed the author's view of conflict may seem rather bland in the 1990s, but the book offers a wealth, if not indeed a surfeit, of materials for studying historical consciousness in the United States.

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JOHN BODNAR. *Remaking America: Public Memory, Commemoration, and Patriotism in the Twentieth Century*. Princeton: Princeton University Press. 1992. Pp. xiii, 296. \$29.95.

When Native American activist Vernon Bellecourt threw blood on a Christopher Columbus exhibit in a Minneapolis museum in May 1992, the director allowed

it to remain and issued a press release explaining its significance. John Bodnar would appreciate this story, since his valuable book explores the ongoing struggle to define the meaning of the American past.

While Eric Hobsbawm and, earlier, W. Lloyd Warner, offered a hegemonic view of patriotic rituals, treating them as instruments by which elites solidified their power, other scholars, including Jürgen Habermas, Clifford Geertz, and David Lowenthal, presented a "multivocal" model in which such observances serve various functions for different social groups. Bodnar reviews this debate and offers his own contribution, stressing the complex role—at once hegemonic and "multivocal"—of public commemorations in twentieth-century America as arenas of struggle between celebrators of the nation-state and "ordinary people." The dispute over the Vietnam Veterans Memorial in Washington, analyzed in the introduction, highlights the themes elaborated in the eight chapters that follow.

Late-nineteenth-century commemorations, Bodnar finds, were shaped by business groups as celebrations of material progress. In the twentieth century, a triumphalist nationalism emerged as the central motif, but never without challenge. For example, Kansas in 1974 observed the centennial of the arrival of Russian Mennonites who brought with them winter-wheat seeds that transformed Kansas' agriculture. Although the "Kansas Wheat Centennial Commission" used the event to promote the state's economy, it held more subtle meanings for the Mennonites themselves, which they expressed in various ways. Bodnar shows how both levels of historical commemoration interacted in complex ways.

He next compares patriotic observances in Indianapolis and Cleveland—the former ethnically homogeneous, the latter an immigrant patchwork—in the 1920s. The commemorative activities in each city reflected these differences: Indianapolis with its "uncontested patriotism," Cleveland with its Cultural Gardens to which various immigrant groups contributed. Each operated within a framework of Americanization and patriotism. Exploring "Memory in the Midwest" both before and after World War II by looking at a series of state centennial observances, Bodnar finds an emphasis on patriotism and cultural authority in the Depression-wracked 1930s, and Cold War consensus-building and renewed stress on material progress, but also a trend toward "greater diversity and tolerance" in the postwar era.

Shifting to the national level, Bodnar sees in the history of the National Park Service an ongoing contest between local boosterism and an official agency whose "basic narrative" traces the rise of the nation-state. The Civil War Centennial and the American Revolution Bicentennial, he suggests, similarly revealed the tension between a nationalistic official emphasis and far more heterodox vernacular observances. While the Republican-dominated American Revolution Bicentennial Administration insisted,

in the words of its director, that the patriots of 1776 did their work so well that "one revolution has been enough," Jeremy Rifkin's People's Bicentennial Commission celebrated the nation's radical heritage. (Senator James Eastland demanded an investigation.) A series of corporate-sponsored bicentennial events contrasted with hundreds of local observances of a richly suggestive diversity. In his conclusion, "Subcultures and the Regime," Bodnar sums up his argument: the growth of the nation-state decisively shaped historical memory in twentieth-century America, but "vernacular culture" vigorously survived in the interstices of official celebrations.

Bodnar has made an insightful contribution to a lively area of contemporary cultural studies. Despite a tendency at times to portray the "ordinary people" as an undifferentiated oppositional group resisting "official" culture, he more typically emphasizes the diversity of interests within the vernacular culture. The research is extensive, although mainly in published works. At times one wishes for more information on the inside processes by which contests over the meaning of the past were negotiated and resolved. The relevant secondary literature is generously acknowledged. Michael Kammen's *Mystic Chords of Memory* (1991) and Edward Tabor Linenthal's *Sacred Ground: Americans and Their Battlefields* (1991), which explore some of the same issues, appeared too late for Bodnar to take their arguments into account. The prose hardly sparkles, but by and large it is workmanlike and conveys the author's meaning.

This book is tinged with a note of loss. The statuary in Cleveland's Cultural Gardens is "defaced with graffiti." The planners of modern-day commemorations increasingly settle for commercial hype and Hollywood-style flash. Anyone interested in the social meaning of public ceremonials will want to come to terms with this thoughtful book.

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ALAN DAWLEY. *Struggles for Justice: Social Responsibility and the Liberal State*. Cambridge: Belknap Press of Harvard University Press. 1991. Pp. x, 538. \$27.95.

An odd thing about the recent spate of calls for "synthesis" in American history is that a large number of synthetic works have been and continue to be written. Thus, Alan Dawley's book, conceived in part as a remedy to the problem of disintegration, takes its place on a shelf of books that pull together American social and political history from the late nineteenth century through the first few decades of the twentieth century. Nonetheless, Dawley has an ambitious agenda. Like a number of recent syntheses, his book brings together recent work on women's, labor, immigration, and African-American history within the context of politics and foreign affairs. It seeks as well

to use a comparison between conditions in the United States and Germany to clarify the development of the state in America. This book also traces the crisis of nineteenth-century liberalism as it failed to provide a framework for governing a diverse society and highly industrialized economy. Were that not enough, Dawley hopes his book will reach professional, student, and general audiences with a dramatic story. Not surprisingly, he comes closer to meeting some of these admirable goals than others.

Two major themes organize his book. For Dawley, the "imbalance between modern society and the existing form of the state" (p. viii) and the increasingly forceful demands of labor, women, and African Americans for social justice provide the most compelling ways to make sense of the seemingly chaotic social and political changes between the 1890s and 1930s. Following those themes, he divides the book into three sections. The first, covering the years from the 1890s until World War I, describes the *laissez-faire* liberal state, its encouragement of the accumulation of wealth into a few hands, the challenges to that state from feminists, unionists, and African Americans, and the use of the liberal state to beat back those challenges. The second section describes the mounting conflict caused by the contradictions of the liberal state and the strains of American involvement in world politics through the middle 1920s. After a brief but important attempt to reform American government spearheaded by a coalition of middle and working-class Americans, ruling elites returned to *laissez-faire* economics and social repression. The contradictions of the state and the demands of social movements were resolved during the late 1920s and 1930s, the subject of the book's last section. Although both Herbert Hoover and Franklin Roosevelt sought to control the business cycle—a task made imperative by the Great Depression—Roosevelt established programs that formed the basis of America's welfare state. Yet this state aimed to provide security, not justice, to its members. Despite its stability, this state, too, contained contradictions that have left the goal of social responsibility still unmet.

Dawley covers most of the subjects one would expect. His focus on the relationship between the state and society and the course of social movements allows for the integration of subjects—most notably challenges to Victorian sexual mores and popular culture—often left out of such works. Dawley also develops a number of important insights, especially the idea that a coalition between elements of the working class and middle class accounted for much of the success of early twentieth-century reform. Yet many of his interpretations are unsurprising. He resurrects the venerable conflict-versus-consensus debate to claim that American society experienced both. Returning to the debate about whether the New Deal was radical or conservative, Dawley suggests that Roosevelt's programs saved capitalism. Dawley could have developed his own insights and questions rather

than rehash these subjects. Specialists might be upset with the treatment of specific topics. Dawley's decision to emphasize the careers of female sexual radicals to tell the history of feminism, while downplaying other women activists, might be controversial. African Americans too frequently appear here as sensual, earthy challengers to strait-laced, white, middle-class culture, an image that outraged some African-American reformers. Meanwhile, the idea that the state and society were out of balance in the late nineteenth and early twentieth century strangely recalls the earlier works of Robert Wiebe and Samuel P. Hays—strangely because Dawley sees his book as a rebuke to their understandings of the period. If those two historians were guilty of failing to acknowledge pervasive conflict during the first third of the twentieth century, then Dawley gives us the struggle of abstract nouns. Here the "New Woman," "New Negro," other "social forces," and "the state" battle over such things as "social reproduction." The melodramatic and metaphorical quality of Dawley's prose detracts from making such struggles concrete. In the end, I suspect Dawley is right: the history of liberalism and a comparative perspective will be crucial parts of a reintegrated narrative of American history. This book, perhaps because of its many ambitions, will probably leave those searching for synthesis still looking.

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SANDRA SCHACKEL. *Social Housekeepers: Women Shaping Public Policy in New Mexico, 1920–1940*. Albuquerque: University of New Mexico Press. 1992. Pp. ix, 213. \$29.95.

This tightly focused study shifts the history of gender and the American welfare state both geographically and chronologically. By working at the state level, Sandra Schackel is able to identify reform initiatives by women that are often overlooked in national studies and to trace the impact of federal policies on local welfare provision. By moving beyond the Progressive Era (where so much recent research has been concentrated) into the interwar period, Schackel can address questions about the impact on women of the professionalization of health and social work and integration into newly expanded state bureaucracies.

New Mexican women formed their own clubs, lobbied for the establishment of a state welfare agency, and worked with numerous federal agencies and programs. They repeatedly crossed the boundary between the voluntary and state sectors, seeking public funding for their own programs and providing private funds, personnel, and ideas for public ones. Indeed, according to Schackel, women's voluntary organizations "bolstered the existing welfare system" (p. 110). Curiously, Schackel's conclusion downplays women's initiative by arguing that "the welfare structure was forced to seek assistance from volunteer

organizations" (p. 164). Her evidence, however, suggests that the impetus for cooperation often came from the women's organizations themselves.

Attuned to New Mexico's variegated ethnic and racial composition, Schackel is careful to include black and Hispanic women's associations in her discussion, while noting that the tribal structure of Native American communities precluded the formation of autonomous female organizations. She challenges the notion that Hispanic women were not "joiners," pointing out that church-based groups such as sodalities taught them how to speak in public, run meetings, and mobilize resources. Unfortunately, Schackel treats each ethnic or racial group only in a vacuum, without probing their interrelations. She never asks whether black women formed their own clubs because they were barred from white ones, or why white women appeared to have had far more influence on government policy than women of color. The discussion would have benefited greatly from comparison to Nancy Hewitt's recent work on female reformers in Tampa, Florida, an area with a somewhat similar ethnic and racial mix.

Schackel is more aware of the racial and ethnic tensions between welfare providers (mostly Anglo) and clients (primarily Hispanic or Native American). She is at her best when describing encounters between these groups in rich detail. She explains, for example, how Elizabeth Forster, a visiting nurse sent to a Navajo reservation in the early 1930s, won the community's trust by showing respect for their medicine men, thus allowing them to accept her ways without surrendering their own. Forster's methods, however, made her suspect in the eyes of Bureau of Indian Affairs (BIA) officials, who were bent on "modernizing" the reservation by driving out native traditions, and she was dismissed.

Schackel's analysis does not always keep pace with her evocative description. In the case of Forster, she misses an opportunity to tease out the gender-laden professional issues that were also at stake between an autonomous nurse who functioned like a doctor and the male physicians of the BIA. Elsewhere, Schackel praises the sensitivity and flexibility of Forster and other welfare workers, like the male Agricultural Extension agents who taught women sewing and canning and the female officials who tailored WPA programs to the plastering and weaving skills of Native American women. These examples cry out for a nuanced critique of the social control model, but none is forthcoming.

While stressing continuities between women's reform activities of the 1920s and 1930s and those of the preceding decades, Schackel misses significant changes. Professionalism, for example, created a new status and identity for the women who went to nurse in remote Hispanic or Native American communities or headed state agencies. These women were not following a traditional script, as Schackel implies,

even if they were primarily concerned, as their voluntary predecessors had been, with the health and welfare of mothers and children.

This study brings us into new terrain but does not offer an adequate map. Although frequently insightful, Schackel at times lapses into near hagiography. Still, her research is compelling and will no doubt prompt other historians to investigate women's state-level activism during the interwar period.

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CRAIG L. SYMONDS. *Joseph E. Johnston: A Civil War Biography*. New York: W. W. Norton. 1992. Pp. xiv, 450. \$24.95.

At last, the field of Civil War history has a thoroughly researched and judicious biography of General Joseph E. Johnston, written by Craig L. Symonds. No prominent Confederate leader has suffered more from a lack of scholarly analysis, largely because controversy followed Johnston from beginning to end in the 1860s. Previous writers have gone to extremes to praise him or to censure him. A good case can be made either way.

Johnston came from Virginia's landed gentry. He was the first West Point graduate to attain a general's rank in the U.S. Army, and as quartermaster general his military contacts were almost unprecedented. As the highest ranking officer to enter Confederate service, Johnston naturally expected to be the ranking field officer. President Jefferson Davis thought otherwise.

The short but handsome Johnston, with piercing eyes and sharp goatee, was the only man who successfully led both of the South's principal armies. He commanded the Army of Northern Virginia until seriously wounded at the battle of Seven Pines in May 1862. After his recovery, Johnston went west on Davis's orders. In time Johnston achieved the command of the Army of Tennessee. His strategy of stabbing at William T. Sherman's army as he retired with his forces to the defenses of Atlanta incurred Davis's anger, and the president removed him from command. In February 1865, Johnston returned to lead what was left of the once-great western army.

Significantly, "Uncle Joe" had the distinction of being the victor in the Confederacy's first and last major military successes (first battle of Manassas and Bentonville). It is what happened in between and thereafter that has left Johnston in an aura of historical dispute.

That Symonds spent years in preparing this biography is clearly evident from the depth of research and the incisiveness of judgments. What emerges so clearly here is the portrait of a capable but proud and sensitive general whose assets could not match his liabilities.



Personally brave, esteemed by the men in the ranks, and often inspiring, Johnston had strategic concepts that would have been a safe, if not solid, policy for the Confederacy to pursue. He was brilliant in his ability to organize, motivate, and maneuver an army. A thoroughly dedicated soldier, he had no political ambitions.

Yet Johnston consistently avoided battle à la George McClellan; not one of his four victories could be termed decisive. Johnston displayed a strong penchant for strategic withdrawals, and in such retrograde movements he too often abandoned badly needed supplies.

Johnston also had a short temper; he could be bright and cheerful one day, sullen and troublesome the next. Sharp sensitivity to rank and army chain of command were likewise not constructive qualities.

Worst of all, Symond accurately states, was Johnston's feud with Davis—a vendetta that both men carried to the grave. Their arguments and disagreements were directly responsible for the breakdowns in the western campaigns of 1864, and their postwar literary salvos at one another did credit to neither party.

Symond has written a first-rate work that will be the standard source on Johnston for many years to come.

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E. STANLY GODBOLD, JR., and MATTIE U. RUSSELL. *Confederate Colonel and Cherokee Chief: The Life of William Holland Thomas*. Knoxville: University of Tennessee Press. 1990. Pp. xiii, 205.

More than thirty-five years ago Mattie U. Russell completed her dissertation on William H. Thomas, a North Carolinian sometimes called the "White Chief of the Cherokees." E. Stanly Godbold, Jr., has expanded Russell's groundbreaking research into an admirable account of Thomas's activities. Left in his mother's care by his father's accidental death, Thomas began clerking at age thirteen in several stores in western North Carolina. Early in his career, he was befriended by the Oconaluftee Cherokee leader Yonaskuga, who adopted Thomas into his clan. Because of Thomas's slight stature, the Cherokee named him Wil-Usdi, or "Little Will." By 1831 the Lufty Cherokee asked Thomas to represent them in Washington.

For thirty years Thomas labored to defend his adopted people against removal and to obtain financial settlement for them. From the time of his first trip to Washington in 1836 he established himself as a successful negotiator. Personally his service to the Cherokee often was less profitable, since he served for years without pay, a chronic condition partly responsible for his later financial ruin. Unfortunately, critics such as the Baptist missionary Evan Jones accused him of pocketing thousands of dollars. Nevertheless, at Yonagaska's death in 1839, Thomas

became chief of the Oconaluftee Cherokee. By 1846 he secured a treaty that allowed the Cherokee to remain in North Carolina and to collect a small per capita payment.

Knowing the necessity of land for Cherokee survival, Thomas spent much of his time and the income derived from his businesses buying thousands of acres, some for himself, the balance for the Cherokee. Land near Quallatown was given to the Cherokee, even though Thomas continued to hold the title in order to protect the Indians from unscrupulous promoters.

From 1848 to 1860 Thomas also was involved in politics as a member of the North Carolina senate; then he was elected to the state's secession convention. Although he was permitted to organize a Confederate unit called Thomas's Legion, his military inexperience and his numerous political encounters rendered his service negligible. Some of his difficulties may have arisen from early manifestations of the mental instability that would haunt his later years, a progressive deterioration the authors blame on syphilis.

After the Civil War, Thomas's life was marked by lawsuits, hospitalization for mental illness, and financial ruin. His most fruitful period had been his service as Cherokee chief and agent. Thomas's persistence helped the Oconaluftee Cherokee remain in North Carolina; his land purchases in behalf of the Cherokee laid the basis for today's Qualla Boundary held by the Eastern Band.

The authors deserve our thanks for their treatment of a complex personality who lived on the frontier of two cultures. Some critics may question the book's lack of attention to the Cherokee side of Wil-Usdi's life; for purposes of context, interested readers should consult John Finger's *The Eastern Band of Cherokees, 1819-1900* (1984). Godbold and Russell, however, have completed their goal of tracing Thomas's amazingly busy career. Despite Thomas's shortcomings, the Cherokee council officially recognized him in 1874 as their "steadfast friend and protector" (p. 140).

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STUART MCCONNELL. *Glorious Contentment: The Grand Army of the Republic, 1865-1900*. Chapel Hill: University of North Carolina Press. 1992. Pp. xvii, 312. \$32.50.

In 1952, Mary R. Dearing published *Veterans in Politics*, a detailed study of the Grand Army of the Republic's (GAR) role as an arm of the Republican Party and lobby for veterans' pensions in the last third of the nineteenth century. Now, forty years later, Stuart McConnell has written a social history of the GAR "from the bottom up," which nicely complements Dearing's magisterial effort. Examining care-

fully the membership and activities of an elite urban area, a working-class small city, and a rural post, McConnell situates the GAR in the context of the numerous fraternal organizations that flourished in the Gilded Age. It provided charitable services for veterans and communities while enabling ex-soldiers to relive their glory days through periodic meetings and "encampments." Achieving its peak membership in the 1880s and 1890s, the GAR justified the pensions it obtained from both states and the nation—which rose to one-third of federal expenditures by 1900—on patriotic grounds, contributing to the hero worship of the Civil War veteran.

The GAR's most important contribution to the United States, McConnell argues persuasively, was not the pensions with which it saddled taxpayers but the instillation of a cult of conservative patriotism. Composed primarily of white middle and upper-class males from German, English, and Irish ethnic groups, the GAR equated loyalty to the nation with law and order, respect for the flag—it gave out thousands to schools and other places—and appreciation of the nation's history, especially the sacrifices of Civil War veterans. The GAR took the lead in turning the Civil War into an example of collective national valor. It eventually formed ties with Confederate veterans' groups while ignoring black Union soldiers, who had a troubled existence in segregated posts at the GAR's periphery.

It is difficult to question McConnell's fine research and thorough description of the GAR's inner workings and place in American civic culture. With few exceptions, historians have not paid much attention to the voluntary associations of middle-class white men in the national past. These associations have not generally been important as instigators of great events or as rebels and reformers—literally, they have fallen in the middle. I would question, however (and here lies my only caveat with McConnell's book), the tendency to see the GAR and similar twentieth-century groups such as the American Legion as nostalgically tied to an irrelevant past, vainly trying to stem the tide of an increasingly diverse and conflicting society. Instead, I would maintain that the continued appeal of a nationalist message not that far removed from the GAR's into our own time—including the popular enthusiasm for Civil War history—has to be explained, and that the GAR and its successors constitute a large part of the explanation.

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Ogontz

ROBIN W. WINKS. *Frederick Billings: A Life*. New York: Oxford University Press. 1991. Pp. x, 398. \$27.95.

Frederick Billings, a successful lawyer and railroad entrepreneur, is hardly a household name. Robin W. Winks makes a strong case that a life of such first

lieutenants of industry can provide us with a range of insights into a changing America in the years from 1840 to 1890.

A native of Vermont and educated at its state university, Billings went to California with the first wave of forty-niners. As a lawyer, he made a considerable fortune in San Francisco, pursuing the practice (common to lawyers and law firms at the time) of accepting lands and mining claims in payment for legal services. The legal barriers to confirming land grants (especially those issued by Mexico) and mining claims were enormously complex, and Billings's firm was the most successful in the state in such litigation. He also traveled to Europe to seek investment in the New Almaden mine, and at the outbreak of the Civil War he worked for the Union cause as a purchaser of arms. Winks sketches each of these shifting contexts with consummate skill. His account of Billings's trip to California with the forty-niners is one of the best accounts we have of the dangerous trip across Panama; his analysis of Billings in the San Francisco of the 1850s—whether the tangled land law, legal practice, vigilante justice, an evolving society—illuminates our understanding of the rise of that "instant city."

Billings and his family left California in 1865 and returned to the East Coast. There, "circumstance, opportunity, surplus capital, and friends" drew him to railroads (p. 182). America was on the threshold of the transcontinental railroad age. It was a challenge that attracted wealthy and energetic entrepreneurs. Billings bought his first stock in the Northern Pacific (NP) in 1869. Winks describes this line and its proposed route to the West as "the single greatest American corporate undertaking of the nineteenth century" (p. 187). For Billings, it was a "splendid adventure" (p. 205). Winks clearly outlines the many twists and turns of the railroad business: stocks and bonds, competing interests of the directors, lobbying in Congress, and a land department that controlled a land grant equal to about 10 percent of the continental land area (p. 187). Billings's contribution lay in salvaging the NP in the aftermath of the great crash of 1873, and in adopting new policies to move forward construction and the sale of lands. These successes led to his election as president in 1879, but he would serve only two years before being ousted by the intrigues of Henry Villard.

In the course of his presidency and afterward, Billings—always interested in nature—promoted the establishment of the Yellowstone area as a national preserve. His interests were not entirely philanthropic, for he wished to use the park to attract tourists who would travel there over the NP and its branch lines. When railroads were forbidden in the park, Billings adopted a sensible mixture of conservation and commerce, but if the two came into conflict, "commerce came first, whether for good or ill" (p. 292). In the end, Billings and the NP played an important role in the creation of Yellowstone, even if their motives were colored with self-interest.

In the final analysis, what commands our attention here is not the activities and wealth of Billings but the great skills of Winks. He has written a splendid biography.

MALCOLM J. ROHRBOUGH  
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W. BERNARD CARLSON. *Innovation as a Social Process: Elihu Thomson and the Rise of General Electric, 1870–1900*. (Studies in Economic History and Policy, The United States in the Twentieth Century.) New York: Cambridge University Press. 1991. Pp. xxii, 377. \$44.50.

This is the second full-length biography of Elihu Thomson. The first one was published in 1944, seven years after his death, and commissioned by his family. The title David O. Woodbury gave his book told everything about its tone: *Beloved Scientist: Elihu Thomson, a Guiding Spirit of the Electrical Age*. As W. Bernard Carlson says in the introduction to his own book, Woodbury praised Thomson's "personal manner, his scientific curiosity, and his genius, but explained little about how and why Thomson went about his work as an inventor" (p. 2). This is what Carlson has sought to do, and it is a mark of technological history's maturation in the past fifty years that he succeeds so much better than Woodbury.

Born in England in 1853, Thomson emigrated with his parents in 1858, graduated from Central High School in Philadelphia in 1870, taught chemistry there for six years, then formed a business partnership with Edwin J. Houston, a one-time teacher of his at Central. There were failures at first, but, after Thomson devised a commercially viable arc-lighting system, a Massachusetts shoe entrepreneur named Charles Coffin provided backing and in 1883 Thomson became chief engineer of the Thomson-Houston Electric Co. in Lynn. Unlike Thomas Edison, he stayed away from finance, sales, and management; rather, he devised the inventions that facilitated a continuous expansion of the firm's product line: incandescent lighting, motors, meters, street railway equipment. Like Edison, he took out an extraordinary number of patents, 696 all told. By the early 1890s, Thomson-Houston, Westinghouse, and Edison General dominated the electrical supply industry, and the 1892 merger of Thomson-Houston with Edison's firm brought forth an industrial giant, General Electric.

That much one can learn from Woodbury, although not as well as from Carlson. But Carlson also has more interesting matters on his mind. He has been influenced by the entrepreneurial school of business history; by the systems approach of Thomas Hughes (his mentor); by Alfred Chandler's analyses of the role of managerial strategy and business structure; by John Law's concept of the "heterogeneous" nature of the social, economic, technical, and political

components of engineering; and by Brooke Hindle's studies of the thought processes required to design artifacts. "Emerging from this rich body of historical scholarship," Carlson writes, "is an awareness that it is no longer satisfactory to view technological innovation as a narrow technical enterprise; the task is to understand how creative inventors, managers, and engineers link these varied activities together" (p. 12).

The strengths of Carlson's book lie in its perceptive attention to Thomson's shifting motivations and its lucid descriptions of his inventions (an effort much enhanced by the selection of nearly 100 illustrations). But the book is analytically rich as well, with two themes adroitly interwoven. One concerns the title, *Innovation as a Social Process*. It is not exactly true, as Carlson implies, that he is breaking new ground in depicting innovation "as an activity in which the inventor interacts with a variety of people" (p. 350), but he has certainly done a fine job of showing how Thomson's backers and business associates matched technology to marketing opportunity, and how his inventions and "the ambient business organizations mirrored each other" (p. 352). A second theme, concerning the role of craft knowledge in modern technology, is less successfully developed. Scholars have always found it difficult to analyze tacit knowledge, and it seems to me that nobody is likely to do very well at this who does not personally possess highly developed "fingertip" skills, and who, as Carlson says, has not absolutely transcended "a longstanding bias among scholars that places a higher value on affairs of the mind than actions of the hand" (p. 5).

At "affairs of the mind," however, Carlson has done admirably. He concludes that broad analytical categories imposed by scholars, such as social constructionism, need to be leavened by due regard for the role of contingency. As historians of technology have rejected naive hagiography and embraced novel theoretical insights, it is well to bear in mind Carlson's final injunction: that interpreting the interplay of technology and culture is still partly a matter of "looking closely at the individual actors and their decisions" (pp. 361–62).

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ROBERT PERLMAN. *Bridging Three Worlds: Hungarian-Jewish Americans, 1848–1914*. Amherst: University of Massachusetts Press. 1991. Pp. xii, 302. \$39.95.

Hungarians have tended to fall between the cracks in studies of Jewish immigrants, partly because of their small number and partly because of their rapid acculturation into American life. In this study of the approximately 100,000 Hungarian Jews who emigrated between 1848 and 1914, Robert Perlman traces the migrants from Hungary to their new lives in the United States as Hungarians, Jews, and, finally, as Americans.

In one of his more interesting chapters, Perlman describes the influence of political conditions in the home country on postmigration attitudes. Unlike their Russian and Polish co-religionists, who had little to do with non-Jewish emigrants from Eastern Europe, Hungarians were active along with Gentiles in Magyar associations. In labor unions, they were less radical than other Jews, possibly because they had held a more secure position in Hungary.

There are some quirks in choice of material emphasized. Despite the fact that only 5 percent of Hungarian Jews emigrated before 1880, Hungarians are compared generally with the German migration, most of which took place during the earlier period, rather than with the contemporaneous Russian-Polish one. Two pages are devoted to the influence of Benjamin Szold as a rabbi who favored the religious reform movement, yet only two sentences discuss the career of his daughter Henrietta, a more important historical figure who was a major Zionist organizer and the founder of Hadassah, the women's Zionist group.

There are also some errors concerning Russian and Polish Jews. Perlman states that the Jewish "Enlightenment"—the *Haskalah*—made little headway farther east because ghetto living "restricted Jewish interaction with Christians" (p. 57). This would have been news to the majority of Jews for whom making a living often required continuous interaction with their Gentile neighbors. And in urban areas, home to the majority of Jews in the Pale, the influence of the *Haskalah* can scarcely be considered insignificant.

More seriously, however, the book gives an incomplete picture, largely because, with few exceptions, men's lives and concerns are treated as the norm. For example, in a brief study of three upwardly mobile Hungarian families from the earlier immigrant generation, Perlman mentions that many married Christians. It would have helped to know whether outmarriage was practiced by both sexes equally, or by men alone, as was often the case with middle-class German Jewish immigrants, with the women marrying Jews and thus acting as cultural conservators. He does not seem to realize that the "gushing, blushing woman's diary" (p. 107) he cites is a rich source for understanding the network of relationships whose hub was the Hungarian Jewish community of Nashville. Perlman mentions but does not account for the preponderance of women in the migration to New York (41,401 as against 35,224 men). In his discussion of life in Hungary, he considers only men's work. Had these young women, who worked in factories in New York, also earned money before migrating, as many Eastern European women did? A letter from one woman immigrant indicates that this was indeed the case, but this aspect of Hungarian life is not explored.

Occasionally there is some confusion, depending on sources used. At one point, Perlman cites a rabbi who in 1887 decried the loss of religion in America (p. 188); at another he writes of the immigrants'

reluctance to abandon their orthodoxy. Perlman might have done well to seek information from sources other than the religious elite whose authority was being undermined by modern trends at the same time in urban areas of Eastern Europe. In short, this is a useful, but limited, study.

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DAVID HARRINGTON WATT. *A Transforming Faith: Explorations of Twentieth-Century American Evangelicalism*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. ix, 213. Cloth \$32.00, paper \$12.95.

David Harrington Watt argues in this brief book that the resurgent evangelicalism that caught the public's attention in the 1970s was by no means "a disciplined and charging army . . . trying to drastically reform American society." Rather, it looked more "like a group of Americans . . . trying—quite successfully—to fit in" (p. 13). In the decades after the 1920s, evangelicalism and, to a lesser degree, its more aggressive sibling, fundamentalism, were reformed by culture into shadows of their crusty pasts.

Watt begins with an impressionistic chapter on Bill Bright's widely circulated pamphlet, "The Four Spiritual Laws," finding in it the subtle compromises with modern thought that are the theme of his own book. He then sketches the history of conservative Protestantism from the 1920s to the 1950s, a period when that subculture divided into clear-cut fundamentalist and evangelical camps. Watt next surveys the rift in fundamentalist and evangelical attitudes toward politics, noting that both groups were deeply concerned about the secularization of modern culture. In these early chapters the author's conclusions are fairly conventional and rely heavily on previously published scholarship.

The last four chapters of Watt's book offer support for his thesis. In "The Private and Public Spheres" Watt notes a declining evangelical interest in dispensational premillennialism, the prophetic theory that had been central to the movement in the early years of the twentieth century. In this subtly argued essay, Watt theorizes that the evangelical expectation of the Second Coming of Christ was replaced by "hopes associated with the family" (p. 11). He then explores evangelical reactions to the changing role of women in American society. Watt demonstrates that evangelicals accommodated some of the ideas of modern feminists and describes the birth of an evangelical counterfeminist movement that challenged feminism while supporting a more moderate understanding of the role of women. He concludes by exploring evangelical attitudes toward modern psychology. Evangelicals in the 1920s and 1930s considered Sigmund Freud one of the archdemons of modern thought, but by the 1970s "the evangelical mainstream was



infused with themes and practices that were rooted in modern psychology" (p. 12).

Watt's case is not always crystal clear. Defining evangelicalism is no easy matter. Watt tries; the book includes an appendix that clarifies his use of such terms as "conservative Protestant," "Evangelical," and "Fundamentalist." But in the text his careful definitions sometimes get lost. When his thesis demands a particular voice to make a point, Watt stretches his definition of mainstream evangelicalism to include spokespersons as varied as hard-core fundamentalist John R. Rice and Lutheran counterfeminist Judith Miles.

Even so, one can not quibble with Watt's arguments or his conclusions. This is a good book, well written and persuasive. It provides another patch for the twentieth-century evangelical quilt. Watt confirms what evangelist Bob Jones suspected about evangelicals decades ago—they played footsie under the table with the world, and, after a while, they unabashedly tried to date her.

DAVID EDWIN HARRELL, JR.  
Auburn University

RANDI JONES WALKER. *Protestantism in the Sangre de Cristos, 1850–1920*. Albuquerque: University of New Mexico Press. 1991. Pp. x, 163. \$27.50.

When Anglo-Protestant missionaries entered northern New Mexico and southern Colorado, along the slopes of the Sangre de Cristo Mountains, in the wake of the Mexican War, they confronted an established Hispanic-Catholic population. The resulting efforts of mainline Protestants from 1850 to 1920 to evangelize the local population were largely ineffectual, although the Presbyterians and Methodists experienced some success. Carefully researched and clearly written, though a little repetitious, this fine study by Randi Jones Walker offers a convincing explanation for the ultimate failure of the Anglo Protestants. Endnotes, an adequate index, a brief appendix, maps, and charts enhance this work's value.

Walker's central thesis is that Anglo Protestants, whose efforts climaxed about 1910, were basically unsuccessful because of a failure to appreciate the Hispanic and Catholic culture of the New Mexicans. Fueled by deeply rooted images of Hispanic Catholics as corrupt, lazy, superstitious, and ignorant, Anglo Protestants, driven perhaps as much by patriotic fervor as religious zeal, entered the Sangre de Cristos bent on Americanizing and Christianizing the locals. This decidedly anti-Catholic and ethnocentric message had scant appeal. As the New Mexicans saw it, the faith of the Anglos could not be fully embraced without becoming de-Mexicanized. Walker also attributes the limited success of Anglo Protestants to the revitalization of the local Catholic church, which was begun in the 1850s under Jean Baptiste Lamy and given momentum by the arrival of the Jesuits in

the 1860s. By conducting revivals, establishing schools, and issuing a weekly newspaper, the Jesuits effectively countered Protestant activities.

Despite the odds, Protestant missionaries labored diligently. Their missions basically were of two types: those organized under the auspices of a missionary society but without organizational ties to the denomination (Baptists, Congregationalists, and early Presbyterians and Methodists), and those established within the organizational framework of the denomination (later Presbyterians and Methodists and the United Brethren in Christ). The latter approach was the more successful of the two, for it incorporated New Mexican churches into the fabric of denominational life. Additionally, the two bodies, Presbyterians and the United Brethren, that admitted Spanish-speaking churches into their regular organizational structure were the most successful. When the Methodists separated their Spanish missions in 1884, the effect was detrimental both financially and numerically.

By 1920, as mainline Protestant activity ebbed sharply, other groups, notably Mormons, Seventh-day Adventists, and Pentecostals, primarily Assemblies of God, entered the Sangre de Cristos. Unfortunately, Walker's story ends in 1920, leaving the study of these more recent arrivals to others. Even so, this is a worthy addition to the literature about churches on the American frontier. And as Walker ably demonstrates, the southwestern frontier was quite different from that of the Ohio Valley, where Anglo-Protestants had been generally triumphant in promoting God and country.

JOHN W. STOREY  
Lamar University

RICHARD WHITE. *"It's Your Misfortune and None of My Own": A History of the American West*. Norman: University of Oklahoma Press. 1991. Pp. xix, 644. \$39.95.

Richard White's weighty volume palpably illuminates the dramatic shift taking place in the historiography of the American West. Shifting sharply away from Frederick Jackson Turner and the frontier (neither is mentioned here), White focuses on the West as region, from initial European contacts to the present. Employing clash-and-conflict themes, emphasizing diversities, and stressing numerous changes over time, the author avoids the romantic and triumphalist tone of many earlier narratives and employs the realistic and critical—if not dark and somber—tones of the so-called New Western historians who have charged to the forefront of revisionist interpretations of the West in the last few years.

White divides his long volume into six parts. The first and briefest section covers initial contacts between Europeans and Native Americans and the earliest imperial designs in the West. Parts two and three, encompassing nearly half of the book, treat the

enormous influences of the U.S. government in the nineteenth-century West and the numerous transformations and developments taking place in that century. Particularly probing is his examination of the centers and peripheries of power defining the West from the 1880s into the 1920s. The final two parts cover the Depression to the late 1980s. Throughout these sections, White draws on numerous recently published essays and books about the West, even while he makes good use of his own premier work on environmental, racial/ethnic, and bureaucratic topics.

As we have come to expect from White, his book is rife with provocative and illuminating insights. The most extensive treatment now available of ethnic, class, and environmental influences on the nineteenth and twentieth-century West, his magisterial work likewise demonstrates how ubiquitous government policies and power, urbanization, and corporate capitalism have shaped—and often abused—the American West. White also supplies revealing discussions of the ways in which national and even international policies and events influenced the western past. Clearly, no one has written more interpretively than he of this larger West through its more than two centuries of history.

Like Patricia Nelson Limerick and Donald Worster, whose views parallel many of those advanced here, White is a superb stylist. His writing is clear, well organized, and conceptual. Even when he deals with traditional topics of the nineteenth century, such as farming, ranching, and land policies, he casts those familiar subjects in new interpretive frameworks bound to intrigue scholars and students alike. Employing attractive similes and metaphors as well as exhibiting appealing humor and irony, the author's story will also draw readers looking for a new general overview of the West.

White's extraordinary emphases on social change and political economy, although challenging and thought-provoking, are not without their costs, however. Stressing changes and diversities among the West's varied classes and its racial and ethnic groups, the author sometimes is so analytical and conceptual that his narrative sacrifices drama, liveliness, and flesh-and-blood humans for clinical studies of political, economic, and social transformations. Additionally, a few sections are not entirely satisfactory. White misses the nuances between fundamental and evangelical Christians, seems to overlook most possibilities of rascality, corruption, and misguided actions among nonwhites, and omits or treats glancingly many elements of traditional cultural history: novels and historiography, paintings and music, and religious and educational trends. The final chapter on the "imagined West," while provocative, is much too brief (15 pages of text) for an adequate treatment of the mythical West.

Still, these are small limitations in a magnificent history. More than three generations ago Frederic Logan Paxson was awarded a Pulitzer Prize for his

*History of the American Frontier* (1924). Twenty-five years later Ray Allen Billington won wide acclaim for his massive synthesis *Westward Expansion* (1949). White's extraordinary volume ranks with these earlier achievements and deserves similar recognition. Even though some readers may fault his omissions and his overemphasis of pet topics, all should recognize this volume as the history of the American West for our time. It richly deserves the critical attention, classroom adoptions, and wide reading it will receive.

RICHARD W. ETULAIN  
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JULIE ROY JEFFREY. *Converting the West: A Biography of Narcissa Whitman*. (The Oklahoma Western Biographies, number 3.) Norman: University of Oklahoma Press. 1991. Pp. xvii, 238. \$24.95.

Julie Roy Jeffrey's excellent biography of Narcissa Whitman, missionary wife of Oregon Territory's Dr. Marcus Whitman, is a valuable contribution to western history, women's history, and Native American history. In fact, the care with which Jeffrey has woven together the disparate strands of many lives enables the reader to empathize with them all. Both Indian and white dissatisfaction and misunderstandings were present from the beginnings of the controversial American Board of Foreign Missions (ABFM) station established by the Whitmans at Waiilatpu in 1836. Unresolved and festering, the disagreements finally resulted in disaster and death.

Settling near Fort Walla Walla in what is now Washington State, the Whitmans thought they were bringing religious salvation from certain damnation, along with the superior benefits of white civilization and agricultural techniques, to the proud, independent, and ambitious Cayuse hunters and gatherers. The Cayuse perceived them as white invaders, living in growing material abundance on Indian land with the help of reluctant Indian labor. The Whitmans were even encouraging more and more emigrants to cross the mountains and settle on Cayuse grazing and hunting lands. Furthermore, the Whitmans often quarreled with the other missionaries who had come with them, and all the evangelical Christians angrily resented the greater success of nearby Catholic missionaries who did not require their converts to undergo the strictly disciplined breaking of the will that was part of the evangelical revival experience.

Jeffrey's reconstruction of Narcissa's childhood casts light on the character of many nineteenth-century evangelical women. Equally valuable is Jeffrey's informed historical imagination in assessing Indian behavior, even though most of the sources are from missionaries and other whites. Narcissa Whitman herself was unprepared for the realities of missionary life, says Jeffrey, and by 1841 she had recognized that fact, trained as she was in a "judgmental emotional Christianity" by parents who

stressed "harsh justice" and inflexibility (pp. 114–15). But both Whitmans blamed greed, Catholicism, and outside agitators as tensions mounted and violent episodes should have warned them away.

By 1845, John McLoughlin of the Hudson's Bay Company was already advising the Whitmans to move to the more settled Willamette Valley, for their own safety. But even when a devastating measles epidemic broke out in 1847 and five or six Indians a day were dying, the Whitmans still refused to leave. The "headmen met in Council" (p. 217) and agreed that the doctor must be put to death in retaliation for his supposed poison "medicine," which was blamed for the deaths of more than 200 Indians. The massacre took place on November 29th, and forty-seven survivors were held captive for a month. Ironically, it was a Catholic priest who came that evening to try to make peace—too late. He performed a Latin funeral service the next day for the Whitmans and ten others. Jeffrey concludes that the Cayuse were the ultimate martyrs, however, when they were subsequently hunted down and their way of life destroyed.

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ROBERT KELLEY. *Battling the Inland Sea: American Political Culture, Public Policy, and the Sacramento Valley, 1850–1986*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xxi, 395. \$35.00.

Robert Kelley has written an important book about environmentalism and public policy. The central theme concerns several generations of farmers and townspeople, joined by engineers, politicians, and judges, who wrest control from nature of one million acres along the lower Sacramento River in order to create a "Holland in California."

Citing Herodotus's conviction that history is a human story, Kelley first recounts the story of people trying to fulfill the American dream against a formidable river. Much of the story centers around their underestimating the problems, costs, and seriousness of trying to change nature, while concurrently overestimating their knowledge, know-how, and ability to work together. Taming rivers is a familiar story. Kelley's contribution is that his command of the sources dealing with the thoughts and actions of these people, and his mastery of this local terrain, leads to an extremely compelling narrative.

Recognizing Thucydides's belief that history also should tell large truths about public affairs, Kelley links local and national political culture and public policy theory to this human struggle. This is an ambitious objective. Kelley's thesis is that between the 1860s and 1900s Whig-Republican ideology gained ascendancy over Democratic ideology. Thus, the principles of localism, volunteerism, and laissez-faire gave way to an expanding sense of community forged

by active state and national government on behalf of an entrepreneurial elite. With regard to the river, this ideological contest led to the implementation of the Sacramento Flood Control Project. The book's major achievement is tying developments in political ideology with the emergence of an engineering elite, centered in the Corps of Engineers, committed to taming the river through a regional plan of dam and levee construction.

In terms of public policy theory, Kelley subscribes to the "policy sciences" (multidisciplinary) approach to understanding human behavior. In criticizing traditional policy theory, Kelley uses river reclamation to expose the weaknesses of models tied to explaining historical developments primarily through rational behavior and self-interest. He devotes considerable attention to demonstrating how chance, timing, preconceptions, learning, memories, and prejudice, among other characteristics, also shaped the ability of these people to interact with this environment.

The ideological framework of this book will be the focus of debate over its merits. Kelley has uneven success in showing relationships between national political culture and the lives of valley leaders and residents. Where the limitations of available evidence come into play, some will see tenuous interpretive connections made between ideology, self-interest, and all of the other factors that motivate human behavior.

As the federal Omnibus Water Act of 1992 made clear, today's escalating battle with the inland sea might be more successful if the lessons learned from the nineteenth-century battle with the inland sea were better understood. Kelley gives us cause for some hope.

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ERIC ARNESEN. *Waterfront Workers of New Orleans: Race, Class, and Politics, 1863–1923*. New York: Oxford University Press. 1991. Pp. xii, 353. \$39.95.

In this book, Eric Arnesen offers a detailed chronicle of how New Orleans waterfront workers negotiated the complexities of race. Arnesen focuses on the white urban workers, free people of color, and ex-slaves on the docks as they dealt with the issue that has been so divisive in the development of organized labor in America. His underlying theme is that an analysis which poses race and class as opposites is not useful; rather, he proposes that race and class must be considered as components of a more complex identity. He maintains that biracial alliances developed on the New Orleans docks in large part because black unions were strong enough to prevent white workers from "imposing a racially exclusionist solution" to unemployment and competition on the riverfront, and because the city's Democratic Party ma-

chine was at least tacitly receptive to dock workers' efforts to build strong unions (p. ix).

Arnesen shows New Orleans to have been a distinctive southern city that supported integrated labor unions and independent black unions made up of cotton screwmen, longshoremen, freight teamsters, cotton yardmen, railroad freight handlers, and roustabouts. He takes due note of the impact of politics on waterfront workers and also explores the hierarchies among them, the differences between the lives of rural and urban black workers, and class divisions within the black community.

The author shifts back and forth from the particulars of New Orleans to the dynamics at work in the larger society. Developing his themes chronologically, Arnesen takes readers through the failure of Reconstruction and explores the extent to which waterfront workers were able to maintain biracial alliances, aided at critical moments by the Knights of Labor. In spite of the rising tide of white supremacy, he writes, the waterfront unions in New Orleans knew they had to avoid racial divisions in order to control the labor supply and, thereby, the docks.

Arnesen describes "an era of interracial cooperation and labor solidarity" that ended in 1892 as the economy slid into depression, accelerating the growth of white supremacy. After that, "the cracks in the color line that extended into labor's ranks began to close" and the craft distinctions that had been relatively dormant on the New Orleans waterfront were revitalized (p. 118). He then explains how interracial alliances were reconstituted in 1901 as New Orleans workers—like those in other parts of the country—challenged managers and owners for control over the workplace. He takes note of syndicalist impulses among waterfront workers and evaluates the effect of ideological strife on race relations. Arnesen concludes with an examination of the impact on New Orleans workers of World War I, an event that brought government intervention in labor relations, and that the unions viewed as strengthening the business-government alliance against labor.

Arnesen's research is extensive. The most striking aspect of his sources, aside from their number, is the prominence of local newspapers, particularly those published by and for black communities. They add considerable weight to a solid and thought-provoking chronicle.

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DOMINIC A. PACYGA. *Polish Immigrants and Industrial Chicago: Workers on the South Side, 1880–1922*. (Urban Life and Urban Landscape Series.) Columbus: Ohio State University Press. 1991. Pp. xiv, 298. \$40.00.

Scholars on the trail of the Polish immigrant invariably find themselves walking under the long shadows cast by the Chicago school of Sociology and by William I. Thomas and Florian Znaniecki's monumental five-volume study, *The Polish Peasant in Europe and America* (1918–20). Dominic A. Pacyga in his study of Polish workers in Chicago is no exception, since much of the evidence and literature compiled by the Chicago school is integrated into his work. Whereas Thomas and Znaniecki built a sociological foundation which argued that Old World communal relationships disintegrated in such harsh urban industrial environments as Chicago, Pacyga contends that the historical evidence demonstrates that migration, conflict, and urbanization solidified the communal tradition of numerous urban villages in Chicago. Moreover, unlike his mentors in the Chicago school who noted an absence of labor activism and radicalism in the Polish working-class community in Chicago, Pacyga has unearthed sufficient documentation to challenge the Chicago school thesis or "any attempt to reduce the American urban working-class experience to a uniform one" (p. 9). Instead, Pacyga argues that the Polish working-class communities fostered long-term stability and strength due to an impressive social infrastructure built from within: by the Catholic church, by an extended family network, and by working-class associations and fraternal organizations. In addition, this communal solidarity also was due to persistent efforts by Polish leaders in behalf of "extracommunalism"—a reaching out to other working-class ethnic areas outside the Polish group.

In his initial chapters, Pacyga discusses the origins of the Union Stockyards and the South Works, which came to attract thousands of working-class Poles. The "new economic system" developed by entrepreneurs in the meatpacking and steel industries contributed to the formation of "worker's communes" in the Polish section of Packingtown and "The Bush" adjacent to the South Works. These "communes" formed the bedrock of the Polish response to industrial dislocation, where death and physical injury in the workplace could come at any moment. Anchored by a community network of self-help organizations at St. Michael the Archangel and Immaculate Conception parishes in the South Works area, and at St. Joseph and St. John of God parishes in Packingtown, the Poles courageously fought off social disintegration in their primary areas of settlement. Eventually, Pacyga tells us, this powerful alliance of church, family, and working-class community was later to gain additional support from outside agencies, namely from the settlement houses established by the University of Chicago and Mary Eliza McDowell, and from labor unions such as the Amalgamated Meat Cutters and Butcher Workmen and the Stockyards Labor Council (SYLC). Unfortunately, during the "years of crisis" (1918–22)—encompassing the Steel Strike of 1919, a bloody race riot in that same year, and the Packingtown strike ending in 1922—the interethnic working-



class coalitions formed in the precrisis period suffered a series of organizational defeats. Bitter feuding between the Amalgamated and the SYLC, each of which included powerful Polish labor leaders (Alex Nielubowski of the Amalgamated and John Kikulski of the SYLC), fractured the strength of the Polish rank and file and contributed to bitter strike defeats in the immediate postwar period in Chicago. Moreover, Kikulski and his immediate successor, Stanley Rokosz, were murdered in separate incidents, robbing the Polish rank and file of vital union leadership.

As Pacyga reminds us, despite this season of despair the CIO in the late 1930s would use the valuable communal village base in Chicago Polonia to rekindle the fires of labor activism. By then, most union organizers learned from sad experience the sharp distinctions between ethnic neighborhood and labor community that Pacyga rightly emphasizes. Scholars who have followed the recent scholarship of Elizabeth Cohen's *Making a New Deal* (1990) and of Robert A. Slayton's *Back of the Yards* (1986) will wish to study Pacyga's valuable monograph in more detail.

JOSEPH J. PAROT  
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JOHN PATRICK DIGGINS. *The Rise and Fall of the American Left*. New York: W. W. Norton. 1992. Pp. 432. \$22.95.

By turns abrasive and poignant, sweeping and microscopic, John Patrick Diggins's study is, above all, a serious engagement with the ideas that have motivated American socialists, communists, and leftists of all stripes throughout the twentieth century. A revised and greatly expanded version of his 1973 volume, *The American Left in the Twentieth Century*, this new book, like its forerunner, is primarily an intellectual history of these movements. As such, those readers looking for social analysis of rank-and-file motivations, actions, and commitments will be disappointed; but if readers engage Diggins on the author's own terms, they will be richly rewarded.

Diggins identifies four distinct periods in the Left's intellectual history. The first he terms the "Lyrical Left," an era bound by the Great War and the hopes of the Bolshevik Revolution, and identified politically with the Debsian socialist movement. Then came the Old Left of the American Communist Party and the Great Depression when, Diggins writes, its predecessors' "sensuous emotions" were eclipsed by a "sodden intensity" born of that decade's anxieties (p. 164). Following a postwar hiatus, Diggins notes the emergence of a New Left. Contrary to many of its historians, however, Diggins perceives little sustained continuity between Old and New; indeed, if anything, he argues for a stronger connection between the Lyrical and the New Lefts. Finally, there is the contemporary Academic Left, ensconced in universities and popu-

lated by many former New Leftists. This Left, Diggins states harshly but not erroneously, has little presence beyond the campus and represents "an idea whose time has passed and whose power has come" (p. 298).

Diggins defines these lefts by their own common denominator: to a greater or lesser extent each embraced a Marxist framework. As a consequence, Diggins perceives a sharp disjuncture between American protest movements of the nineteenth century and these lefts. But he also argues that an intellectual commitment to Marxism did not therefore insulate the Left from the influence of the broader political culture. Central to this analysis is the continued importance of individualism.

Throughout the book, Alexis de Tocqueville, Ralph Waldo Emerson, and, to a lesser extent, Walt Whitman form a leitmotif in at least two ways. Diggins uses insights from these writers to contrast the experience of the Left, evoking in the process explicit explanations of the Left's failures. Most debilitating, of course, if broad social change were to occur, was the Left's inability to form a sustained alliance with working people, Marxism's traditional agent of historical progress. More interesting, Diggins explores the core of individualism at the very heart of much of these self-professed Marxist proscriptions. In the Lyrical Left of Jack Reed and Randolph Bourne, in the New Left of Herbert Marcuse, and in the Academic Left of various deconstructionist tendencies, Diggins reflects on the unreconstructed "I" at the core of the more formal collectivist pronouncements. For the Old Left, most prominent in a period of great social tension, the fact remained that, for all the protest, working people nonetheless responded to the Depression more with "patience and a sense of contrition" (p. 146) than with a mobilized collective identity.

The limited successes that the Left did attain reinforced rather than challenged progressive tendencies within American politics. Diggins sympathetically treats New Deal reform, civil rights, opposition to the Vietnam War, and the emergence of a strong feminist Left critique, among other Left contributions. Paradoxically, however, Diggins sharply insists that what long-term gains the Left did achieve came, notwithstanding his discussion of individualism, as the result of the remnants embedding themselves within the modern bureaucracy, be it governmental, academic, or among the foundations. This bureaucratic mentality gives a painful, Weberian cast to earlier claims for a more transcendent collective consciousness.

Diggins reserves his most biting commentary for the contemporary Academic Left, "the first Left in American or European history to distrust the eighteenth-century Enlightenment" (p. 347). Although he devotes far too many pages to dissecting this tendency, his central point is worth serious discussion. In professional historical work as in contemporary politics, Diggins looks to revive a liberal skepticism that

can critique both the exuberant positivists and the innocents who would posit any power as inimicable to the true human condition. The important insights Diggins developed in his earlier book *The Lost Soul of American Politics* (1984), he now brings, in telescoped fashion, to the history of the American Left with results that are original, provocative, and important.

Many will disagree with Diggins's ideas; others will balk at his often acidic formulations. Yet Diggins does perceive intellectual and political value in the Left. If he is short-tempered with the Left's mythologies, and appalled at its current anti-intellectual linguistic turn, it is precisely because Diggins takes the Left's own historical traditions quite seriously. "A Left without power is familiar," Diggins warns, "and perhaps a defining characteristic of its historical predicament; a Left without knowledge loses its excuse for being" (p. 16). Intellectually and politically, Diggins suggests, that makes us all the poorer.

NICK SALVATORE  
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STEVE BABSON. *Building the Union: Skilled Workers and Anglo-Gaelic Immigrants in the Rise of the UAW*. (Class and Culture series.) New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xii, 298. \$50.00.

Steve Babson's thoroughly researched and finely crafted monograph argues that two overlapping groups, tool and die makers and Anglo-Gaelic immigrants, played "prominent leadership roles" in the formation of the United Auto Workers (UAW) in Detroit.

Two anomalies in the development of mass production in auto manufacturing in the early part of the century lay behind the role of these two groups. Babson shows that while most skilled workers disappeared and the percentage of foreign-born auto workers declined, the number of tool and die makers increased, as did the percentage of Anglo-Gaelic workers. Based on the lives of thirty Anglo-Gaelic UAW leaders, most of whom immigrated in the 1920s, Babson argues that they were part of a distinct subculture rooted in British trade union and Irish national struggles. This subculture featured a strong craft and class consciousness, organizational know-how, and a commitment to direct action strategies. In Detroit, this subculture found expression in Odd Fellows lodges, and it enabled those who were part of it to play leadership roles in key auto workers' struggles of the mid-1930s—particularly the Briggs strike of 1933, the formation of the Mechanics Educational Society of America (MESA) in 1933, the first automobile sit-down strike at Midland Steel in 1936, the Chrysler sit-down strike of 1937, and the General Motors tool and die strike of 1939.

Part of the enduring fascination of early auto history is how it happened that large numbers of diverse workers united, waged dramatic and success-

ful struggles, and created a formidable industrial union. As part of an explanation for this phenomenon, Babson's monograph makes a new and valuable contribution.

The author, however, takes a different, less defensible view of his contribution. He argues that a perspective on "the initiating role played by strategic groups" is more a legitimate concern than the "popular myth" of a "unified and spontaneous militancy" (p. 1). That the unity and militancy achieved by auto workers happened spontaneously may be a myth, but that it happened was a fact. By dismissing this Babson tends to exaggerate the uniformity and virtues of tool and die makers and Anglo-Gaelic immigrants and to make invidious comparisons with native American and eastern European workers. To argue, for example, as Babson does, that Anglo-Gaelic workers had greater trade union aptitudes and abilities, greater class sensibilities, and less political gullibility than other ethnic groups simply ignores the leading role played by native Americans and eastern Europeans not only in some of the struggles he describes but also in the Ford drive, the GM sit-down strike, and in all other auto conflicts outside Detroit. Like their Anglo-Gaelic counterparts, other auto leaders usually had something in their background that distinguished them from the masses, whether experience with left-wing politics, other unions, or higher education, but these leaders were not deficient in union aptitudes, abilities, and class consciousness. A book that so enriches our understanding of intraclass dynamics can be forgiven its overreaching claims.

ROGER KEERAN  
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Empire State College

UDO SAUTTER. *Three Cheers for the Unemployed: Government and Unemployment before the New Deal*. New York: Cambridge University Press. 1991. Pp. xii, 402. \$54.95.

The discovery of unemployment as a problem of industrial society led to a variety of early twentieth-century reform proposals that called for a new, more aggressive public role. Increased reliance on government was controversial and reformers made little progress until the Depression of the 1930s, when severe unemployment reduced opposition to government intervention in economic life. The consequences included various state and federal initiatives during the Hoover administration, the New Deal unemployment relief program, the Social Security Act, and a consensus that society has an obligation to prevent unemployment and to compensate victims.

Although Udo Sautter's retelling of the story of unemployment reform contains few surprises, it is an impressive addition to the literature of early twentieth-century public policy and reform activity. Indeed, it is the kind of study that historians increasingly

depend on: the careful synthesis that supersedes the detailed specialist studies that preceded it. Sautter has read everything and has meticulously reconstructed the story of unemployment reform from primary sources and secondary works. His footnotes alone are a valuable reference guide. Most of all, his encyclopedic treatment makes it possible to understand precisely what unemployment reform meant in the 1920s and 1930s.

Although there was comparatively little legislation until the 1930s, the reformers' ideas dated from an earlier period and strongly reflected the influence of the American Progressive movement. Unemployment reform resolutely focused on the failures of the labor market. The reformers' correctives included public employment offices to rationalize the chaotic scramble for jobs, advance planning of public works expenditures to coincide with business cycle downturns, and unemployment insurance to aid the unemployed and penalize employers who contributed to labor market turbulence. Sautter painstakingly traces each of these proposals, emphasizing the cumulative effect of the debates of the 1910s and 1920s and the hesitant public programs of the early Depression years.

Of the reformers' programs, the public employment office was the key. It would count the unemployed, reduce frictional and seasonal unemployment by bringing together "the jobless man and the manless job," and distribute relief and unemployment insurance benefits. Although some states created public employment offices as early as the 1890s, and the federal government introduced a nationwide system during World War I, the results simply underlined the complexity of the problem and the shortcomings of progressive social engineering. Some of the obstacles were political and administrative; others were related to the mobility of the American labor force. Even after Congress expanded and upgraded the system in the 1930s, public employment offices did not live up to the reformers' expectations. The other reforms had similar fates. Advance planning of public works proved to be politically impossible, and unemployment insurance has never outgrown the limitations that practical politics imposed on it in the 1930s.

The reformers' omissions were also noteworthy. Until the 1930s reformers took the economy for granted; no one thought systematically about macroeconomic demand management as an antidote to unemployment. As a result, the Keynesian revolution and the rise of fiscal policy, apart from public works spending, have no part in Sautter's story. Similarly, reformers paid little attention to education and job training. That too was someone else's bailiwick and, if relevant at all, was unimportant compared to the overriding task of bringing order to the labor market. Captive to the concerns and perspectives of the early twentieth century, reformers provided at best partial answers to the problem of unemployment. Sautter

has written a comprehensive guide to their limited agenda. His work is essential to anyone who hopes to understand the intriguing relationship between unemployment and government in the twentieth century.

DANIEL NELSON  
University of Akron

ROGER W. LOTCHIN. *Fortress California, 1910–1961: From Warfare to Welfare*. New York: Oxford University Press. 1992. Pp. xviii, 420. \$45.00.

Roger W. Lotchin has written a big and important book about the "metropolitan-military complex," the term he prefers over the long-fashionable "military-industrial complex." It might well have been a small but important book. Given to repetitious writing and organization, piling on details in dense and sometimes annoying fashion, Lotchin lets the narrative line go slack and the principal arguments become confused. To be sure, his story is by nature untidy, and he rightly captures the complexities. Better editorial control would have yielded a leaner, cleaner, more controlled volume.

Lotchin's study is uneven in other ways as well. He grasps the view from California better than the perspectives of Washington, D.C., and the armed services (for which his secondary works are idiosyncratic and sometimes dated). Gender as a category of analysis, and even women just as historical actors, are almost entirely missing. His laments and barbs about other historians become tiresome. And sometimes he grasps at straw men—and then not with a sure grip. He asserts that his story "refutes [President Dwight] Eisenhower's notion of a military-industrial complex" (p. 41) and flails Ike for implying that there was some "unitary institution" at work (p. 345). In fact, Eisenhower was at pains to point not only to industry and science but also "to every city, every statehouse, every office of the federal government," and to leave open whether their "unwarranted influence" was "sought or unsought."

Impressive research and fresh thinking outweigh such flaws. Lotchin challenges older views about both the substance and the chronology of the complex process of America's militarization. He sees the "metropolitan-military complex" as springing less from some imperial center in Washington than from the periphery—if, in this tail-wags-dog story, "center" and "periphery" even remain useful distinctions. Wars and international struggles provided the circumstances, but urban and regional rivalries, and the imperial aspirations and anxieties of California's urban politicians and boosters—men whom Lotchin treats skillfully, if with angry admiration—decisively shaped the process. Playing secondary roles were scientists and universities, fledgling entrepreneurs and labor interests, and military bureaucrats and officers who shelved partisan and ideological differ-

ences in order to land the bonanza of federal monies. Moreover, their alliances, their successes, and regional conflict over them were already well in place by 1940, before the era most historians see as the take-off phase of militarization, and long before the much-vaunted "Sunbelt-Frostbelt" conflicts of the 1970s and 1980s.

The result has been an ever-changing "polycentric configuration," one "better termed a 'metropolitan-military complex' because cities and metropolises were the fundamental bases of these alliances" (p. 353). If the winners were the "Sunbelt" generally, California's cities especially, and above all San Diego and Los Angeles, it was not because of the "natural" advantages of climate, geography, and free enterprise that the boosters trumpeted. Indeed, Lotchin casts a sharp eye on the ironies, contradictions, and downright dishonesties of Californians who preached peace and bewailed federal power, agreed that "Nothing the wartime government did seemed right" (p. 153), and connived for every penny that government could allocate.

California's success was certainly great; Lotchin "wonders," rather grandly, "if there was ever a time in the history of Western Civilization when the military expenditures of a free-spending great power were so absurdly maldistributed" (p. 234). Why that success occurred remains opaque here, as does whether the "metropolitan-military complex" decisively shaped the scale of national security spending or merely its flow. Lotchin offers many factors and considerations, but no tightly formulated argument about them. But his deeply informed book will challenge modern American historians.

MICHAEL S. SHERRY  
Northwestern University

AUGUST HECKSCHER. *Woodrow Wilson*. New York: Charles Scribner's. 1991. Pp. 743. \$35.00.

August Heckscher is a journalist and public servant who set out to write a one-volume biography of Woodrow Wilson for the nonscholar, and he has largely succeeded. He is a member of the editorial committee supervising the publication of the Wilson Papers, and his work makes available in serviceable prose the narrative gist of that series. Most competing books are either much briefer, aimed at the student textbook market, or much more specialized. If you accept the basic attitude of Arthur Link and his associates, assume that Wilson was a great statesman of high ideals, and you wish to give a book in this area as a gift to a family member, this work will do nicely.

For anyone else likely to consult this journal, the work is of little importance. It does not so much shed light on new sources or on areas of serious controversy as exude a warm glow over what we already know. It seriously overestimates the level of intellectual discourse inhabited by Wilson's father and ex-

cuses away the many inadequacies of Wilson's own thought. It glosses over his provincialism and chronic inability to complete scholarly tasks. It ignores the opportunity seriously to examine Wilson's contributions to political science and history as possible keys to his personality or to the larger discourse of his time. It admits defects in Wilson's personality and behavior, but always with rhetoric that implies that Wilson was not living up to his own ideals, or that his judgment was weakened by illness or exhaustion. Where the authenticity of Wilson's words remains in doubt, as in the notorious Frank Cobb interview, Heckscher paraphrases the Link position and says not a word about those who disagree. The bibliography is seriously defective and neglects many subjects entirely. Always a good liberal, this Wilson stands tall indeed on civil liberties, even if the winds of public opinion or the axe of A. Mitchell Palmer cause a momentary wavering high in the air. This is an honest book and a fair portrait by the lights of the author, but it is not the Wilson many others have found.

Worst of all, it perpetuates the myths of internationalism that have blinkered historians of the subject since Ray Stannard Baker. It makes no effort to view Wilson from the private papers of Henry Cabot Lodge, Robert La Follette, or Hiram Johnson. It pays their arguments no attention either, leaving the impression that most opposition to Wilson was narrow partisanship or personal vendetta. Wilson's vision was noble and heroic, the argument seems to be, therefore his efforts were admirable. One would think that the sorry record of the League of Nations, and the ineffectuality of most activities of the United Nations, not to mention the course of history since Versailles, would have caused some rethinking on these issues. The whole foreign policy history of modern America needs serious revision, and it should start with Wilson's opponents, on both the Left and the Right, and how their views were frequently more sensible than his.

ROBERT M. CRUNDEN  
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Austin

HOWARD BALL and PHILLIP J. COOLER. *Of Power and Right: Hugo Black, William O. Douglas, and America's Constitutional Revolution*. New York: Oxford University Press. 1992. Pp. viii, 390. \$27.95.

Constitutional scholars have long been aware of the contentiousness that characterized the behavior of the justices whom President Franklin D. Roosevelt appointed to the U.S. Supreme Court after the constitutional revolution of 1937. Works such as Wallace Mendelson's *Justices Black and Frankfurter: Conflict in the Court* (1961) focused on personal antagonisms, as well as the continuing debates about judicial activism versus judicial restraint and balancing versus absolut-



ism that have characterized the history of the modern Court. This work by Howard Ball and Phillip J. Cooper is, by contrast, an analysis of the relationship between Justices Hugo L. Black and William O. Douglas who, from entirely different premises, developed and shared an absolutist approach to First Amendment freedoms during the greater part of their three-plus decades of service on the Court.

The authors argue that Black defended constitutional rights and liberties because "he felt the people through the Constitution had commanded it," whereas Douglas did so because "he thought freedom to be prior to government" (p. 4). Thus, the authors contend, "through these two men one can see the effort to play out the fundamental conflict of American constitutional life, the continuing tension between political power and constitutional rights" (p. 9). Their thoroughly researched and, for the most part, clearly written volume provides more than ample proof in support of their thesis. Their depiction of the Black-Douglas relationship does not prove, however, that the absolutist position the justices developed led to the resolution of the continuing conflict between freedom and equality in our constitutional world. One can argue plausibly that their inability to agree on which of the two elements should prevail was a major cause of their eventual split.

Ball and Cooper present separate biographical chapters, another on Black and Douglas's roles as New Dealers, and another on their relationship with their fellow justices, to support their argument that many of the justices' fundamental beliefs were formulated before they came to the Court. Not enough consideration is given, however, to the liberating impact that the death of President Roosevelt and the end of World War II had on their movement toward absolutism. The remainder of the work consists largely of an examination of the evolution and impact of Black and Douglas's decisions during the war years, the second Red Scare, desegregation, due process, religious freedom, federalism, and the demand for "new" constitutional rights. Presenting clear, concise case analysis and carefully selected excerpts from the two justices' decisions, the authors remind us that Black and Douglas paid a price for their beliefs, Black being unable to live in his home state of Alabama and Douglas having to face two attempts to impeach him.

The work is not without flaws. Besides some mud-diness of description and confusion of chronology in describing Black's friends and family relationships (p. 44), and again when discussing judicial allies (p. 99), the study is no more successful in explaining Douglas's seemingly contradictory behavior in the Rosenberg spy case appeal than most recent works. Might we not consider taking Douglas at his word in this matter? The authors' contention that the Black-Douglas split was based on disagreement about the function of the Court rather than ideology seems questionable. Black broke with Douglas, most signif-

icantly, in *Griswold v. Connecticut* (1965) because he could not accept Douglas's willingness to claim a right of privacy based on penumbras (emanations) from the Bill of Rights that did not appear in the literal text of the Constitution. Douglas ultimately proved to be more of an absolutist than Black.

Nevertheless, the flaws in this work are minuscule in light of its superb depiction of the Black-Douglas relationship and the serendipity that marked the exposition of First Amendment absolutism in their case opinions. Ball and Cooper remind us that the justices arrived at this position after two sharply contrasting intellectual journeys. But the most important point that the authors make in this richly researched, interesting volume is that they did arrive, setting a standard of high expectations for the judicial protection of civil liberties that more recent conservative justices could not fully disregard.

JAMES C. DURAM  
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DOMINIC J. CAPECI, JR., and MARTHA WILKERSON. *Layered Violence: The Detroit Rioters of 1943*. Jackson: University Press of Mississippi. 1991. Pp. xvii, 329. \$32.50.

Dominic Capeci, Jr., and Martha Wilkerson's thoughtful study explores the Detroit race riot of 1943, the most destructive of this century until the riots of the 1960s. Sparked by interracial brawling at a park and fanned by rumors, two days of rioting by both whites and blacks claimed thirty-four lives, left nearly 700 injured, and saw two million dollars worth of property stolen or destroyed before federal troops restored order.

Following the riot, politicians, the press, and others in Detroit explained the outbreak with the presence of riffraff and outsiders. Many blamed the riot on recently arrived and supposedly maladjusted and ignorant southern white "hillbillies" and their black counterparts. Some blamed white ethnics. Others claimed that enemy subversives had fostered the riot to disrupt wartime production. Still others pointed to teenaged hooligans.

Capeci and Wilkerson apply the method of participant analysis, which recent scholars of collective violence have used with increasing success. Drawing on police, court, and probation records, they identify many black and white Detroit rioters in surprising biographical detail. They also identify many riot victims, yielding further clues to the rioters' behavior and motives.

The authors find that most black and white rioters were not hoodlums but ordinary, law-abiding, stable, working-class citizens. Recent southern migrants were not prominent in the violence. Neither immigrants nor their offspring predominate. Blacks rioted within the ghetto and were often arrested singly for looting, destroying property, or carrying concealed

weapons. Whites, however, apparently tended to riot in "militia-like" groups that swarmed at the boundaries of the ghetto, assaulted black passers-by, and launched unsuccessful attacks on the black neighborhood.

In examining the motives of individual rioters, the authors describe the multidimensional nature—"layers"—of the violence. They argue that many blacks rioted in frustration over "blocked socioeconomic opportunities, which they attributed to white racists" (p. 58). Their violence expressed dissatisfaction with heavy-handed white police, exploitation by white shopkeepers, substandard housing, segregation, and inadequate recreational facilities. Some exhibited "mindless theft," others simple "bloodlust." Black newcomers to Detroit possibly expressed "cultural alienation and social anomie." Since three-quarters of white businesses attacked were owned by Jews, black anti-Semitism was another possible motive. White rioters wanted to bolster white supremacy, but many allegedly were angry over competition with blacks for employment, housing, and recreational facilities. Teenaged white rioters perhaps were acting out the prejudices of their elders. Some whites may have sought vengeance for minor preriot interracial scuffles. Ethnic participants may have rioted to prove they were truly "American." Some might object that the presence of these motives among the rioters cannot be empirically demonstrated.

A major strength of this study is its identification of riot participants and its detail on riot behavior. One important finding, for example, is that nearly 70 percent of white rioters and over 60 percent of black rioters traveled at least one-half mile from home to engage in violence. This is consistent with earlier work (such as my *The Sociogenesis of a Race Riot: Springfield, Illinois, in 1908* [1990]), which shows that strangers pose the greatest danger during riots, while those with ties forged through stable residential proximity or other regular contacts are unlikely to participate. In short, this book is a welcome addition to the growing literature on twentieth-century interracial conflict.

ROBERTA SENECHAL  
Washington and Lee University

RICHARD B. FINN. *Winners in Peace: MacArthur, Yoshida, and Postwar Japan*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xxi, 413. \$35.00.

The author of this history of the U.S. occupation of Japan had a unique opportunity to observe firsthand the dramatic events he writes about. Richard B. Finn arrived in Japan as a naval officer in the autumn of 1945 and returned as a member of the foreign service in 1947. He capped a distinguished diplomatic career by serving as the State Department's director of Japanese affairs in the late-1960s. Yet this is no

memoir. Finn has written a well-researched if somewhat traditional monograph focusing on the roles played by prominent Americans and Japanese, most notably Douglas MacArthur, Yoshida Shigeru, and Emperor Hirohito, during the occupation. His narrative relies on U.S. and British documentation supplemented generously by Japanese memoirs and interviews.

Readers familiar with the scholarship of John Dower, Howard Schonberger, Marlene Mayo, and others will find relatively few surprises in this volume. The narrative follows a generally chronological format that highlights the planning of the occupation, the operation of MacArthur's headquarters, the reform programs implemented before 1948, and the Japanese political response to foreign occupation. The treatment of the origins of Japan's postwar constitution is especially interesting and contains significant new material.

In contrast to much recent scholarship, Finn emphasizes the cooperative aspects of the occupation, showing how Japanese leaders and Americans worked together to chart a liberal course for Japan. Aside from disruptive elements on the Left, the author applauds the record of political achievement. Finn downplays the importance of MacArthur's political aspirations, does not attribute much significance to the general's conflicts with the Truman administration, and pays relatively little attention to the challenges raised by Japanese conservatives and radicals to the American agenda. While acknowledging that a "reverse course" (or conservative trend) predominated after 1948, Finn attributes this to the desire in Washington to cut occupation expenses and to assure that Japan would be a stable and self-sufficient nation. He takes issue with scholars who argue that U.S. planners primarily hoped to develop Japan as the hub of an active, anticommunist alliance in East Asia.

Finn's account is richest for the period through the middle of 1950. He devotes relatively little attention to the dramatic impact of the Korean War on Japan's economy and domestic politics, nor does he develop the theme of Japan's emergence as the pivot of the U.S. security network in East and Southeast Asia. Nevertheless, this is an informative and thoughtful addition to the field of occupation studies.

MICHAEL SCHALLER  
University of Arizona

BRIAN BALOGH. *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power, 1945-1975*. New York: Cambridge University Press. 1991. Pp. xi, 340. \$34.50.

Brian Balogh assumes in this book the ambitious task of explaining the course of American policy making since 1945, a period he believes has been either ignored or seriously misunderstood. In his view, the

postwar period marks the third great epoch in the nation's politics. The first revolved informally around the courts and political parties of the late nineteenth century. The second, or Progressive, era coincided with the rise of the administrative state, which was staffed by a new generation of disinterested experts. The third and latest stage began in World War II and can be called the "proministrative state" (p. 12). It was this development that saw a permanent link between self-interested federal agencies and experts who needed each other in equal measure and who determined policy from the top down. This marriage of convenience appears indissoluble despite the current pervasive public distrust of either government's capacity or experts' authority.

Balogh explores the rise and subsequent distrust of the experts in an impressively researched and careful examination of the nuclear industry. As it did in so many fields, World War II transformed the way policy making was devised and executed. The federal government's Manhattan Project that developed the atomic bomb earned scientists, engineers, and professional administrators a position of unprecedented public confidence. But this lofty status, even mystique, lasted no more than a generation as these experts moved into the companion field of commercial nuclear power.

In the beginning, policy making for commercial nuclear power remained the province of a few government officials and scientific experts in engineering and physics. Nuclear experts had rushed into the waiting arms of government officials who promised a politically insulated environment for research and who had the bureaucracy to implement their findings. Together, these civilian officials and experts institutionalized their tight control over nuclear power in the newly established Atomic Energy Commission, chaired first by David Lilienthal and then by Lewis Strauss, and the Joint Committee on Energy in Congress. Public policy was thus shaped by expert and administrative agendas. But whereas government desire, scientific expertise, and government agencies existed to boost nuclear power, consumer demand did not. Private companies understandably showed little interest in investing enormous sums to build expensive nuclear reactors, preferring that the government—usually the military—fund risky nuclear development projects. The government recognized corporate reluctance and decided to stimulate marketplace interest itself in a move that typified the postwar period.

The problem was that the government had to engage in extensive salesmanship to promote its sometimes questionable objectives. In so doing, the government's experts generated an equal and opposite reaction from contending forces concerned about their own particularistic issues, such as safety and environmental protection. Gradually the public became convinced that national security was just one of many concerns of importance. As a result, policy

making became much more complex and diffuse as competing experts from other scientific fields challenged in-house experts.

By the 1970s, the policy-making debate had been transformed into an old-fashioned power struggle over participation and rights. This transformation sacrificed policy coherence and undercut the very consensus that experts needed to exert programmatic influence. The result was a political Catch-22. Experts were always in demand for their authority, but the more they became involved the less credible they seemed. Nevertheless, no substitute for expertise has yet been found.

Balogh argues that the story of trivialized experts and fragmented policy discourse is being replayed today in fields beyond nuclear power. The public has lost further confidence in the sky-is-falling experts who warn almost daily that life itself is being threatened by any number of potential catastrophes. He mentions radon in homes as one such "cry wolf" episode (p. 323). He concludes with some sage advice for government officials: open the policy-making process fully in the early stages and "test for demand rather than seek to create it artificially" (p. 326).

Like most interpretive works, this one undoubtedly overplays its hand and is occasionally difficult to follow, but it cannot be ignored. Balogh makes a substantial contribution to the field of public policy.

BRUCE J. DIENFIELD  
Canisius College

NATALIE ROBINS. *Alien Ink: The FBI's War on Freedom of Expression*. New York: William Morrow. 1992. Pp. 495. \$27.50.

Over eighty years have elapsed since the FBI began snooping on America's writers. The Bureau intercepted their mail, tapped their phones, and collected information about their political activities and private lives. The evidence, some 150 files collected by Natalie Robins under the Freedom of Information Act (FOIA), offers a chilling perspective on the world of J. Edgar Hoover. It also reveals the benefits and disadvantages of using the FOIA.

As Robins herself admits, the FBI is hardly forthcoming. Not only does it take years for the Bureau to process documents but the materials it does release are almost always incomplete. Many documents are withheld in their entirety, ostensibly on the grounds of privacy, security, or the protection of a confidential source. Much of the material that is released is so full of deletions that it is essentially useless. At ten cents a page for photocopying, a large FOIA request can be expensive. The government is supposed to waive the fees for people whose research is in the public interest, but it does not always do so.

Theoretically, one can appeal the deletions and the refusal to grant fee waivers, but that requires a lawyer and a lot of time. During his thirteen-and-a-half-year

quest for the FBI's files on the academic community, Sigmund Diamond noted that he and the FBI exchanged 1,718 pieces of correspondence. Robins decided to forego that kind of litigation and so had to work with what she recognized are incomplete sources.

The problems of doing so are enormous, especially since Reagan administration Executive Orders have made it much harder to obtain material. At the moment, the government no longer even has to disclose whether the requested file exists. Not only are researchers being deprived of material, but they do not even know what they cannot get. In such a situation, it is hard to evaluate the documents that do emerge. It is also easy to inflate their importance. Simply obtaining an interesting or intelligible page or paragraph is a major achievement—and a temptation to emphasize the information that has been so arduously obtained.

As a result, there is a tendency, in Robins's book as well as in much of the other recent literature on the FBI, to offer juicy snippets from the files as if the mere fact that the FBI collected the material tells the story. But these only tell part of the story. Without a broader context it is difficult to assess the significance of the FBI's operations. Robins, for example, asserts that Hoover put writers under surveillance because he was hostile to free expression. A more plausible hypothesis, however (and one that Robins's evidence supports), is that the FBI investigated writers, just as it investigated other Americans, when they engaged in the type of left-wing political activities that Hoover abhorred. Both Robins and Herbert Mitgang, whose book *Dangerous Dossiers* (1988) also summarizes writers' files, note that the G-men rarely read their targets' books.

Robins is unsure about the impact of the FBI's surveillance on the writers she studies. Certainly the Bureau contributed to, if it was not entirely responsible for, the problems that Kay Boyle, Josephine Herbst, John Steinbeck, William Carlos Williams, and Nelson Algren encountered in getting or keeping government positions and passports. Other writers who seem to have escaped direct injury nonetheless felt themselves under surveillance, an anxiety-provoking experience if nothing else. Still others seem to have been unaffected. Were they tougher, more conservative, or was the FBI not as powerful as it may have appeared?

There were other agencies in the field. Howard Fast was sufficiently notorious as a public spokesman for the Communist Party in the 1950s for mainstream publishers to reject his manuscript of *Spartacus* without FBI intervention. In addition, the FBI was not omniscient. It made errors, and, deletions aside, it also missed important pieces of its subjects' careers. Its files, for example, contain no indication that columnist Murray Kempton belonged to the Young Communist League in the 1930s or that Norman Mailer ran for mayor of New York in 1969.

Yet despite the problems that FBI files present, writers like Robins do historians a real service. More a primary source than a coherent analysis, this book, despite its many factual errors, is nonetheless valuable for indicating the scope of FBI surveillance—and its persistence. Under the "Library Awareness Program" of the 1980s, the FBI tried to persuade librarians to hand over information about the borrowing habits of their foreign clients. The ensuing public outcry seems to have forced the Bureau to retreat, but Robins remains suspicious. The duplicities of the past make it hard to believe that the FBI has abandoned its illicit ways.

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THOMAS D. SCHOONOVER. *The United States in Central America, 1860–1911: Episodes of Social Imperialism and Imperial Rivalry in the World System*. Durham, N.C.: Duke University Press. 1991. Pp. xiii, 253. \$32.50.

This is a prodigiously researched, clearly written, and provocative collection of nine essays that addresses such diverse topics as Confederate diplomacy, Pan-Americanism, Theodore Roosevelt's handling of the Panama affair, and U.S. efforts to overthrow governments in Nicaragua and El Salvador. Thomas D. Schoonover characterizes his study as an "appeal for greater openness to the theory and methodology" necessary to place United States foreign relations in the context of "international history" and to understand the "cultural values" of other societies (p. 11). Building on intellectual foundations constructed by Fernand Braudel, André Gunder Frank, Immanuel Wallerstein, and William A. Williams (among others), he employs the constructs of world-systems theory, social imperialism, and *dependencia*. He buttresses these conceptual approaches with exhaustive research embracing official archives and private collections in Costa Rica, Guatemala, Honduras, France, Germany, Great Britain, Spain, and the United States.

From these perspectives and data, Schoonover argues that the United States advanced from semiperipheral to metropole status during the late nineteenth and early twentieth centuries. Along with this newly obtained position as an industrial and rising military power came wrenching economic depressions and disquieting social dislocation. Together with other metropole powers such as Great Britain and Germany, the United States sought to remedy these difficulties via social imperialism, or "the use of external relations to solve internal problems" (p. 166). The United States's attention fell on Central America because of the area's value as a transportation link to the commerce of the Pacific basin and, as the period progressed, because of the region's intrinsic economic potential. Driven primarily by material considerations that showed little concern for Central American



welfare, the United States waged an imperial battle with the European powers (particularly Germany). Over the course of this struggle, U.S. policy evolved from protecting to actively promoting the nation's commerce; by the eve of World War I, U.S. domination was apparent and the Central American countries were left in a dependent and exploited condition.

Critics of world-systems analysis, social imperialism (or the Open Door thesis), and dependency theory will dispute Schoonover's conclusions. They cannot, however, dismiss his exhaustive research, his careful attention to an often neglected period of U.S. foreign relations, or his persistent effort to place otherwise isolated diplomatic episodes into an intelligible theoretical framework. Still, even if one accepts the author's methodological parameters, some questions arise. Predictably, given the application of these methodologies, Schoonover's Central Americans seem to be most often acted on. Although the Central American perspective is carefully considered, even greater attention to initiatives from the periphery would be useful. Similarly, the treatment of Central American collaborators (*compradors*) frequently leaves them as one-dimensional servants of U.S. interests rather than as often complex figures who pursued national and personal goals within the context of severely constricted choices. And, as is perhaps inevitable with a collection of essays, some of these episodes demonstrate Schoonover's methodology and substantiate his conclusions more effectively than others. For example, the author effectively indicts the morality and legality of Roosevelt's treatment of Colombia in 1903, but the incident's precise place in the methodological design could be more fully explained. These relatively minor reservations notwithstanding, Schoonover has certainly achieved his goal of marrying broad theoretical perspective and multinational research into a challenging and informative treatment of U.S. relations with Central America.

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MICHAEL L. CONNIFF. *Panama and the United States: The Forced Alliance*. (The United States and the Americas.) Athens: University of Georgia Press. 1992. Pp. xi, 201. Cloth \$35.00, paper \$15.00.

This book is the fifth volume in the United States and the Americas series published under the general editorship of Lester D. Langley. Within the general concept of the series, this particular work is a comprehensive and expert survey of the relationship between Panama and the United States. Michael L. Conniff undertakes the task of synthesizing a vast amount of information and a complex set of circumstances, and he succeeds admirably. He demonstrates that although Panama is a small country, it has been a

major concern of the United States and it occupies a unique place in U.S. foreign relations.

This significance stems, of course, from Panama's location at the isthmus between the Atlantic and Pacific oceans, and the construction of the Panama Canal by the United States and the creation of the Canal Zone, a U.S. enclave in the heart of the Republic of Panama. It is fortuitous that Conniff's previous works have dealt with labor and race relations in the isthmian region because he is able to provide a human quality to this study and to transcend the traditional diplomatic history approach. Conniff helps one understand that U.S.-Panamanian relations involved not just the conflict between two countries over revenue and sovereignty but also unequal economic, political, and social relationships among people of various nationalities and ethnic and racial backgrounds. Moreover, because of the distinct interests between the defense and operation of the canal and the general considerations of foreign policy, Conniff points out that the United States rarely spoke with one voice. The U.S. Department of State had to deal with "the governments of the two Panamas—the Republic and the Canal Zone" (p. 85).

Conniff presents these dynamics clearly, but he does not ignore the essential diplomatic aspects, beginning with the negotiation of the Hay-Bunau-Varilla treaty in 1903 and ending with the signing of the Carter-Torrijos treaties in 1977. Panama was dissatisfied with the original treaty before the ink was dry, which led to an unequal partnership and the rise of militant Panamanian leaders such as Arnulfo Arias, José Antonio Remón, and Omar Torrijos, who were nationalists and had in common the demand for canal treaty revision. Even the scoundrel Manuel Noriega, whose activities included "gunrunning, money laundering, and drug smuggling" (p. 152), clung to power by wrapping himself in canal treaty demagoguery. The U.S. role in Noriega's rise, however, reveals much about the nature of its relations with Panama generally.

This is a balanced study. Conniff is no apologist for U.S. policy, but he understands the contradiction between the American perception of itself as a positive force and the reality of its often negative presence. The fact that the great work of the Panama Canal produced only an alliance of expediency and no special bond of friendship is a case in point.

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EDWARD S. MILLER. *War Plan Orange: The U.S. Strategy to Defeat Japan, 1897-1945*. Annapolis, Md.: Naval Institute Press. 1991. Pp. xxi, 509. \$34.95.

War planners in democracies have a difficult job. They must first discover how to defend their nation's territories and interests, and they must then persuade

the nation's citizens and politicians to provide the means to implement their plans. Their success has been decidedly mixed. For example, U.S. strategists spent nearly ten years after 1982 planning how to defend the Saudi Arabian oil fields, but in 1990 a long-standing lack of heavy sealift capacity for U.S. armor opened a ten-week period for Iraqi tanks to roll south and seize much of the world's oil supply. Only Saddam Hussein's bad planning, or failure of nerve, stopped the Iraqi push at the Saudi border and paved the way for an eventual coalition victory.

The reputation of the Orange Plan is as mixed as that of the plan that outlined initial U.S. operations in the Persian Gulf. In a classic article, Louis Morton argued that the Orange planners failed to choose between their desires and strategic reality ("War Plan ORANGE: Evolution of a Strategy," *World Politics* 11 [January 1959], 221–50). Although the naval strategists could not persuade the U.S. government to provide the fortified bases and the ships needed to defend the Philippines, they also would not forsake their Mahanian dreams of naval empire and retreat from the dangerously exposed colony to the safer haven of Hawaii. As a result, the United States failed to deter Japan in 1940–41, and it exposed the Philippines to Japanese conquest in 1941–42.

Edward S. Miller disagrees with Morton's indictment. He believes that U.S. naval planners created "history's most successful war plan" (p. xix), because the Americans' "competitive and untidy" way of planning encouraged open and productive debates (p. 330). In particular, Miller celebrates the rise of strategists trained at the Naval War College, who exchanged their predecessors' plans for a rapid advance to the Philippines for a slower island-hopping program in the mid-1930s—a campaign that tacitly wrote off the Philippines. Miller then argues that the U.S. government followed the main outline of this version of the Orange Plan to victory in World War II.

Miller makes a case for a modest revision of our opinion of the Orange planners, but he overstates his argument considerably. In 1935 the naval strategists did back away from their unrealistic program of a rapid transpacific advance to the Philippines, and in 1938 they postponed such an advance, until the Atlantic was made safe against Adolf Hitler. Thus, Miller may be correct in his assertion that the final Orange plans called for the abandonment of the Philippines, though if he is, then naval leaders seriously misled Douglas MacArthur, the army chief of staff, who would eventually try to defend the colony (pp. 182–87). Miller adds that the Orange planners correctly anticipated a Japanese surprise attack at the outset of the war; that they foresaw the need to bypass Japanese strongpoints, like Truk; and that they predicted the need for amphibious forces to seize forward bases.

The Orange Plan was not all that Miller claims, however. The shift from a rapid transpacific advance

to an island-hopping campaign came very late; for most of its existence, the plan called for a rapid and risky push into enemy waters where the fleet would lack a secure base for refueling and repairs. And, of course, the Americans did attempt to defend the Philippines in 1941 by pouring in scarce air power. Miller's argument that World War II planners followed the Orange Plan to victory seems particularly strained. U.S. leaders officially rescinded the plan before war broke out, and wartime planners did not consult it often. Instead, the defensive Rainbow V Plan was in effect in 1942–43, and thereafter fleet planners worked ad hoc to arrange carrier operations and amphibious landings. In addition, the Orange Plan predicted that Japan would conduct a long campaign of attrition, followed by a decisive battle-ship encounter, but both sides fought a series of battles of attrition between their land and sea based air forces. Nor did the Orange planners anticipate the loose submarine and air blockade that so greatly contributed to the defeat of Japan.

Historians will find much of interest in this exhaustive, although defensive, exposition of a series of offensive war plans, but the better of the argument over those plans remains with Morton.

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JOHN R. LAMPE *et al.* *Yugoslav-American Economic Relations since World War II*. Durham, N.C.: Duke University Press. 1990. Pp. xi, 249. \$35.00.

To anyone watching the death throes of Yugoslavia and the hapless attempts by the United States to come up with some sort of policy toward this event, it is revealing to consider the roots of the U.S.-Yugoslav relationship. This volume by John R. Lampe and others does so in a way that is direct and uncomplicated and that, despite its multiple authorship, is effective as a unit and informative.

Lampe offers a review of the first twenty years of this unique bilateral relationship, wrapped around the theme that this was essentially a marriage of convenience. "Keeping Tito afloat" served U.S. desires to try to weaken the Soviet "bloc" and economic support helped the besieged and then reforming Yugoslav economy. Although students of the period now may not blush at this revelation, Lampe's discussion shows how aiding a communist country at this time still had to be debated in Washington. Who was on what desk mattered and Lampe's presentation of these battles, based on the now public primary sources, is very valuable.

The unique and complicated basis of Yugoslav's "worker self-management" economy is dealt with by Ljubisa Adamovic. This chapter offers a lucid encapsulation of an extremely complex mixed economy.

Observers of contemporary attempts to transform a socialist into a market economy may find it instructive.

Russell Prickett considers Yugoslavia's commercial and financial relations and provides what must stand as the most detailed and informed discussion of bilateral and multilateral debt negotiations available. Most of this is based on this author's own direct involvement in these negotiations.

The volume is more complete and nuanced in its discussion of American policy and actions than on Yugoslavia's policy toward the United States, due primarily to the availability of documentation and the authors' experience. It contains some wonderful historical addenda, such as the fact that New York Mayor Fiorello La Guardia spoke Serbo-Croatian, and is a virtual encyclopedia of data on U.S. aid, trade, and business with Yugoslavia. Its major gap is a lack of sufficient discussion of the context of policy making from the Yugoslav side. Although it is widely considered that Tito reserved foreign policy for himself—and this volume does nothing to dispel that view—more time could have been spent on how relations with the United States were seen by Yugoslavia's leadership in light of the centrality of its policy of nonalignment and its relationship with the Soviet Union. It may be that the full story of policy debates on what must have been a hot issue will have to await the opening of the Yugoslav party archives.

The importance of the Soviet Union to the U.S.-Yugoslav relationship is reemphasized in the book's conclusion, in which the authors note that in the future the Russian threat will likely not be part of the "equation" used by U.S. policy makers in developing policy toward Yugoslavia. While the authors do not say so explicitly, the logic of their argument is that without the presence of that threat the United States may have trouble coming up with a compelling rationale for aiding a troubled Yugoslavia. Indeed.

RONALD H. LINDEN  
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ROLF TAMNES. *The United States and the Cold War in the High North*. Brookfield, Vt.: Dartmouth. 1991. Pp. 384. \$64.95.

This excellent scholarly volume is a thorough and balanced study of U.S. strategic thought and policies affecting the "High North"—mainly Norway—during the Cold War. Rolf Tamnes, a historian and senior research associate at the Norwegian Institute for Defense Studies in Oslo, bases his book on extensive multinational research in American, British, and Norwegian archives, published primary materials, personal interviews, and scholarly secondary accounts. Although the focus is on the High North, on Norway, and on the Northern Flank, the author keeps a proper perspective relative to the total picture.

Tamnes distinguishes between Continental, maritime, and polar strategic perspectives or "mental maps." In geopolitical terms he distinguishes between emphases on hemispheric defense, offensive strike capabilities, and perimeter strategies. He traces constantly changing strategic planning resulting from evolving military technology, intelligence developments, and political shifts. He portrays Norway as torn between its "invitation" efforts to draw the United States into firm security commitments and its "screening" to limit American and NATO penetration that might draw Soviet retaliation. In those terms Norway (and the author) applauded "bridge-building" between East and West, but at the same time they generally preferred John F. Kennedy's Flexible Response and Paul Nitze's forward strategies that left no doubt about American commitment to Norwegian security. The book is highly structured, carefully organized, factual, and clearly written. Nonetheless, with its concentrated factual detail, it is not an "easy read."

Tamnes identifies, explains, and evaluates top level civilian and military bureaucratic planning and reports. He more often refers to the national or NATO bureaucratic units than to specific individuals. Even prominent military and civilian leaders come through in only two-dimensional forms. There is no attempt to examine traditional diplomatic, economic, or cultural relations between the United States and Norway except insofar as they fall within the ideology and geopolitics of strategic planning. Third World and north-south issues receive scant attention.

This book is as nearly definitive on its subject as is possible without research in relevant sources on decision making and planning within the former Soviet Union.

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JAMES J. WIRTZ. *The Tet Offensive: Intelligence Failure in War*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1991. Pp. x, 290. \$34.95.

The Tet Offensive by communist military forces in Vietnam in 1968 remains the subject of historical fascination because it was the turning point of the Second Indochina War and because of its paradoxical character. It was a costly military defeat for the forces that launched the dramatic countrywide surprise attacks and, at the same time, a devastating political defeat for the American war effort on the home front.

James J. Wirtz provides a detailed account of the attackers' preparations and of the intelligence collection and warning by the U.S. and South Vietnamese governments. Wirtz's conclusion, which is summed up in the subtitle, is that Tet was an intelligence failure. Despite abundant warning, the U.S. com-

mand was unable to act quickly enough to forestall the attacks, to take adequate defensive measures in most places, or to warn the American government and public in convincing fashion of the coming onslaught.

The absence of effective warning, Wirtz believes, was crucial to the outcome, for "it was the shock produced by the attacks, not the military consequences," which deeply affected the political will in the United States (p. 275). Although he makes no effort in this work to examine in detail the impact of Tet on the homefront, Wirtz's observation is widely shared and is almost certainly correct.

The strong point of Wirtz's work is his careful, thorough, and resourceful accounting of the information and analysis available in advance about the Tet attacks to the defenders, and of the analyses of the information made by several key officials and agencies. In this, he goes beyond previously available works. Wirtz has mined not only standard works and memoirs but also the abundant information in archives as well as the wealth of material produced as exhibits in the *Westmoreland vs. CBS* lawsuit.

For all the useful information, Wirtz finds it difficult to explain the ineffectiveness of the many warnings. Part of the answer, he recognizes, was the military improbability of the attacks, which violated the principles of concentration of force and seemed doomed and therefore unbelievable as a strategy. Beyond this, Wirtz relies on "the theory of unmotivated biases," suggesting that individuals tend to tune out information that tends to contradict their belief systems.

The contrast between formal dissemination of warnings and reluctance to take them seriously in real life may be the subject for a psychiatrist as much as for a military historian. Most telling is the fact that 200 colonels of U.S. military intelligence headquarters in Saigon spent the night before the surprise attacks at a Bachelor Officers Quarters party in the heart of Saigon with no special security precautions even though their own command had promulgated a "red alert" of potential attacks. Gen. William Westmoreland, the commander of U.S. military forces in Vietnam, and Brig. Gen. Philip Davidson, his intelligence chief, returned to their quarters for the night rather than staying at their headquarters, despite all the warnings they disseminated to others. If they did not believe their own warnings enough to act on them, it is hard to see how they could have been emphatic enough to shake up the White House and the American public.

DON OBERDORFER  
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DANIEL WIRLS. *Buildup: The Politics of Defense in the Reagan Era*. Ithaca: Cornell University Press. 1992. Pp. xi, 247. \$31.50.

Daniel Wirls has written an interesting and important contribution to our understanding of the role of partisan politics in the perception and formulation of U.S. national security policy. Focusing on the massive military build-up of the Reagan administration, Wirls maintains that "coalitions and institutions arrayed for and against the Reagan revolution used their defense policy agendas in their efforts to build or enlarge their constituencies and political influences" (p. 1). To buttress his position, Wirls examines three major defense-related movements, placing each within the framework of the domestic political competition of its time: the Strategic Defense Initiative (SDI), the nuclear weapons peace movement, and the military reform movement. He argues that each movement owed a considerable portion of its origin and continuation to its organizers' search for a viable political movement capable of advancing its proponents' political agenda. For political analysts who normally concentrate exclusively on security issues in an effort to understand the politics of defense spending, this is a novel and sometimes fruitful approach.

The most interesting aspect of Wirls's study concerns the initiation and support for SDI. The author believes that the inception of this project cannot be understood independent of President Reagan's political advisers who, fearing a loss of momentum in the continuing defense build-up due to the nuclear freeze movement, sought a new defensive concept capable of gaining massive public support. And while it is difficult to trace the exact genesis of policy making in the Reagan White House, Wirls combines written sources and interviews to make his point. This section alone would make this book important. Wirls's account of the other major defense movements is equally informative and impressive.

The attempt to link these three campaigns to a conscious effort by individual politicians "to build and enhance their constituencies and political influence" is somewhat more tenuous. It is difficult to believe that Reagan's tenacity on behalf of SDI arose from a desire to fend off the nuclear freeze movement, especially since he remained steadfast long after the movement had ceased to be important. And the breadth and bipartisan nature of the military reform movement belies arguments that it served some common political goal separate from the aim of reformed procurement and rational budgeting. To make these points, one would need evidence from the politicians involved indicating ulterior motives, and such evidence appears to be lacking.

Despite the underproven connection between these specific movements and their initiators' separate political agendas, this work will be of great interest to students of both security and defense planning, and those concerned with domestic politics and coalition building. Wirls has initiated a most fruitful field of



study that casts new light on American politics in the Cold War era.

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#### CANADA

HEREWARD SENIOR. *The Last Invasion of Canada: The Fenian Raids, 1866–1870*. (Canadian War Museum Historical Publications, number 27.) Toronto: Dundurn, with the cooperation of the Canadian War Museum and the Canadian Museum of Civilization. 1991. Pp. 226. \$29.95.

The nineteenth century was a dangerous time for neighbors of the United States. If the government of the republic did not covet their territory, certain of its citizens assuredly did. And chances were that private enthusiasms would meet remarkably little check from public officials. The Tenth Commandment was for other people—so Mexico discovered to its sorrow. Besides the War of 1812, Canada suffered two sets of border incursions: one in the rebellion period, 1837–38, the other in the Confederation years, 1866–70. The Fenian raids of that era are the subject of Hereward Senior's book.

Senior has dealt with the Fenians before. In *The Fenians and Canada* (1978), he covered their origins, development, and disintegration. His latest work examines their incursions into Canada. Here they attempted to win a toehold, hoping to use this to bargain England out of Ireland. They launched a series of raids on New Brunswick, Canada West (Ontario), Canada East (Quebec), and Manitoba. All but one of the raids, that of June 1866, not far from Niagara Falls, were ludicrous affairs. But at Ridgeway some 800 Fenians sent a like number of Canadian militiamen fleeing from the field of battle before skeddaddling back across the border themselves.

Senior tells the stories of these forays well, providing ample detail, plotting movements and positions on maps and piquing interest with well-chosen photographs. He makes a noble attempt to be even-handed, but that he finds the battle at Ridgeway the most fascinating aspect of his subject is evident. Here is his best researched and most spritely written work. Secondary works become truly secondary, accounting for some 5 percent of the notes; elsewhere they hold steady at levels eight times higher.

Senior sees significance in Ridgeway. It gave the Fenians and their supporters a great victory, the first Irish triumph in arms in many years—one that would help keep the flame of Irish nationalism burning brightly. It shocked Canadians out of their torpor and set in motion a series of reforms that made the militia, the country's main fighting force, an effective, if not formidable, entity. As for the other raids, they too had effects, helping to bring the Maritimes into Confederation and thus cementing the new union. These are not controversial judgments.

Senior's book is a good one, succeeding admirably in its main purpose: to provide a clear and effective account of the raids for academic and nonacademic alike. Senior is somewhat muddled, though, in one respect. He seems to imply that the Fenians should have engaged in guerrilla warfare (p. 132); later he is sure that they were wise not to have done so, for American authorities would have acted promptly "on the first overt violation of neutrality laws" (p. 187). Fenians, like their counterparts in 1837–38, were able to secure arms in the supposed safekeeping of authorities, muster on the border in strength, cross that border with those arms, shoot at, terrorize, and occasionally kill their northern neighbors, return back across the border, and take up whatever jobs they had, including, in the case of T. W. Sweeny, that of brigadier general in the U.S. Army. Despite the political difficulties American politicians encountered by upsetting the large bloc of Irish voters, or the near impossibility of beleaguered officials charged with enforcing unpopular federal laws on the margins, the willingness of the American government and its people to curb cross-border brigandage is in doubt.

Senior gives such matters scant attention, for he sees Fenianism as an Irish phenomenon transferred through immigration to North America. He therefore accepts the long-held assumption that the Fenian soldiers of 1866–70 were discharged Irish-American veterans of the Civil War. That, to my knowledge, is a lightly tested notion. What North American rhythms, one wonders, underlay those Fenian incursions into Canada?

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ROBERT C. VIPOND. *Liberty and Community: Canadian Federalism and the Failure of the Constitution*. Albany: State University of New York Press. 1991. Pp. x, 249.

At the outset of his book, Robert C. Vipond correctly notes that for "the past twenty-five years the Canadian polity has lurched from one constitutional crisis to another" (p. 1). This instability is often attributed to profoundly different views of freedom/liberty held by Canadians. On the one hand there are those, often Quebecers, who emphasize the importance of community freedoms, especially minority language rights and the protection of distinctive minority cultures. On the other hand there are those concerned with traditional individual liberties like free speech, due judicial process, equal access to employment, and so on. This clash of views is well illustrated in the Canadian Charter of Rights and Freedoms, added to the constitution in 1982. The charter guarantees an array of rights under such heads as "fundamental freedoms," "democratic rights," "legal rights," and "equality rights." Thus, traditional individual liberties

are constitutionally entrenched. At the same time collective rights are given strong protection. This is done in the charter's famous (or notorious) "notwithstanding clause," which allows the Parliament of Canada or any provincial legislature to opt out of most of the guaranteed charter rights. The "notwithstanding" power has been used most noticeably to prohibit the use of English on certain business signs in Quebec. This caused a furor in Canada and is viewed by many as evidence of a profound (perhaps unbridgeable) constitutional chasm concerning the place of the individual within Canadian society.

Vipond rejects this notion of incompatible views and argues that the two positions represent little more than subtle variations on the theme of liberalism. These variations, he contends, have been present from the beginning and are embedded deeply in Canada's constitutional psyche. Because provincial autonomists are normally the advocates of collectivist values, he chose to study the political thought of one such group. Late-nineteenth-century Ontario provincial rightists, at both the federal and provincial levels, constitute his sample.

The resultant analysis is not very satisfying. In a succession of chapters Vipond takes us through the standard issues: disallowance, reservation, the powers of the lieutenant-governors, jurisdictional autonomy for the provinces, the role of the courts, and the nature of federalism. He rejects the longstanding interpretation that the provincial rightists on these issues were usually Liberals arguing against federal Conservatives for partisan reasons. Instead, they represented an intellectual tradition known as "legal liberalism" and therefore were not mere partisans. Unfortunately, the realities of politics intrude. The most important of the autonomists, argues Vipond, was David Mills: "In my view . . . neither [federal liberal leader Edward] Blake nor [Ontario Premier Oliver] Mowat expounded the provincialist reading of the constitution as comprehensively or exposed its difficulties as clearly as David Mills" (pp. 152–53). And Mills was a tireless (and tiresome) exponent of provincialism. He made countless learned speeches against the federal power of disallowance. But as federal minister of justice after 1897 "he disallowed as many acts in . . . five years . . . as his predecessors in the past had vetoed in the previous ten" (p. 140). Vipond describes this as "curious" and later comments that his shift toward centralism is understandable: "With a Liberal government in charge, the federal government could be entrusted with greater power needed to build the nation, without at the same time, threatening liberty" (p. 190). The partisan interpretation of Ontario's provincial rightists was well argued by J. C. Morrison in his 1961 thesis, "Oliver Mowat and the Development of Provincial Rights in Ontario: A Study in Dominion-Provincial Relations, 1867–1896." Vipond does not really alter Morrison's

interpretation. Canada's constitutional chasm is as much with us today as it was a century ago.

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DOUGLAS COLE and IRA CHAIKIN. *An Iron Hand upon the People: The Law against the Potlatch on the Northwest Coast*. Seattle: University of Washington Press or Douglas and McIntyre, Vancouver. 1990. Pp. 230. \$26.95.

The potlatch, the fundamental institution of the Indians of the North Pacific, has long been the focus of anthropological study. It offered a training ground for many of the elders of the profession and still provides a common reference point for the discipline. Yet it is far less well known to historians, particularly in Canada. Few would even have been familiar with F. E. LaViolette's *Struggle for Survival: Indian Cultures and the Protestant Ethic in British Columbia*, written nearly forty years ago, which offered a limited perspective on the place of the potlatch in the history of native-European relations.

In this new study, Douglas Cole and Ira Chaikin focus on the opposition of the non-native society to the potlatch, and specifically on the use of federal laws from the 1880s onward to ban the institution. Based on the records of missionary organizations and the Department of Indian Affairs, this is a sound professional account by historians who are aware of the limitations and biases of their sources. They are careful to note the varieties of opinion on the potlatch in both the aboriginal and European communities and to acknowledge the ineffectiveness of enforcement. They understand tribal and regional differences and, as historians, clearly portray the potlatch as a changing institution, not a static ethnological construct. Similarly, government policy is viewed as the product of character and circumstance, of the constant Canadian tension of region and center as well as political pressure and ideological choice. Well written and clearly focused, this book has much to offer the student of late-nineteenth and early twentieth-century Canada.

Yet the destruction of aboriginal societies is part of a much larger global process. The self-righteous confidence of the missionary and the drive for regulation, inspection, and uniformity that characterized the emergent bureaucracies of early twentieth-century empire are both reflected here. Canadian history, an introspective discipline, would have benefited from a context of broader international and historical significance.

The greatest challenge, however, lies in the judgments offered. Cole and Chaikin defend the role of the antipotlatch law in attempting to end arranged marriages and other aspects of Indian life that Europeans found objectionable. The authors believe that

the law itself was not evil, although the power and paternalism it reflected could be so considered. Fundamentally they view the issue as a conflict of values and ultimately believe that the Indians' ability to resist meant that the "Indians won at least as often as they lost" (p. 183).

But in the potlatch was encompassed the law of the people of the North Pacific. When, in 1951, Canada returned to the Indians their liberty to practice the potlatch they also began, unknowingly, the long process of restoring sovereignty to First Nations. The 1992 constitutional recognition of the inherent right to self-government of all aboriginal peoples in Canada will create new orders of government with their own laws and practices. In British Columbia the law of the potlatch, with its significance for land, property, and family, will inevitably be part of the new order. Cole and Chaikin's study, a view from outside the aboriginal community, offers new material, solid research, and small "I" liberal opinion. This book is timely but its views are unlikely to be shared by the generation of aboriginal leaders who have borne the burden of the struggle for sovereignty.

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#### LATIN AMERICA

INGA CLENDINNEN. *Aztecs: An Interpretation*. New York: Cambridge University Press. 1991. Pp. xiii, 398. \$29.95.

Disregard the audacious title; this book by Inga Clendinnen has somewhat greater merit than its name suggests. It is not about the "Aztecs," a mythical people who may or may not have served as fountainhead for the real groups who came to inhabit Mesoamerica. Rather, and to be correct, it is about the Nahuas, Native Americans who shared a common language (Nahuatl) and way of life. And most particularly, it is about the Mexica, a group of Nahuas who created and lived on the extraordinary island city of Mexico Tenochtitlán.

Written as an explication of Mexica culture and their exaggerated propensity for human sacrifice and anthropophagy, Clendinnen's book is an agonizing exercise in hermeneutics based on a few ancient Nahua codices, colonial records, and indigenous art and architecture. Creatively overstated while extremely narrow in focus, the work is made up of four somewhat connected essays. The first is about the "City" and its great society. Accordingly, Mexico Tenochtitlán is described as an open theater: visual, dramatic, and exemplary. The Mexica used festivals of killing as performance art to engage, entertain, and horrify themselves and their neighbors. The ceremonies were seen as sacred petitions to deities with conflicted wants and needs who alone, it was believed, controlled Mexica destiny.

In the second essay, "Roles," Clendinnen animates the production by confabulating the activities of the

men, women, and their children who were part of the sacrificial plays. Key were the victims themselves. Taken in war, collected as slaves and tribute, or babies obligated to gods, all were supposedly privileged to be sacrificed, and they seemingly cooperated in the elaborate readying procedures. By some reports, on occasion tens of thousands had their hearts ceremoniously cut out in commemoration of a date or event. Depending on the celebration, ritual cannibalism may have followed.

Likewise, everyone participated in the scenario, whether directly or indirectly, by virtue of being Mexica. Society's professionals—the prideful warriors, ascetic priests, and enigmatic merchants—were the motor for the performances, for politically, socially, and economically they had the most invested and therefore they had the most to lose. This was an exclusively masculine federation, at least publicly, and they expended great effort in theatric posturing to remind everyone of the endlessness of their quotidian obligations to bring into consonance all that was sacred and secular.

Women were close but never apparent counterparts. Socially benign in attitude, they were fabulously empowered personally. Women, as such, were the crucibles for the perpetuation of *Mexicayotl*, "Mexica-ness," their cycles being the impulse for human, yet cosmic, regeneration. Complete as wives and mothers, through the perils and pleasures of birthing and breastfeeding, as high a ceremony as any, they reenacted the spectacle of sacramental sacrifice and cannibalism.

The third essay, "The Sacred," elaborates on aesthetics and ritual. As esthetes, the Mexica believed beauty to be all pervasive; it was central to artistic expression and was felt to be as precious and as transitory as life. To capture sacral reality, native pictorials were rendered as graphic metaphors for Mexica existentialism, and sagacious scribes and poem-singers routinely and eloquently interpreted and historicized for the masses. Yet by 1519, in spite of the Mexica's exhaustive repertoire of performances, there was no interested audience of Spaniards. The final essay, "The City Destroyed," in just a few pages touches on the quick and complete end of the Mexica's two-hundred-year hegemony.

Clendinnen typically writes fine history, but this should not be taken as a signature piece. It is far more literary than historical, and in the untoward course of portraying human sacrifice as the epitomizing event for preconquest Mexica culture, she neglects context and the hundreds of thousands of other Nahuas who had their own grand societies (see Raymond D. Fogelson, "The Ethnohistory of Events and Nonevents," *Ethnohistory* 36 [1989], 133–47). Indeed, the Mexica themselves lived on, well into the colonial period, and without human sacrifice. It is an imaginative work. For want of facts, from time to time Clendinnen stretches to other Native Americans' practices and conjectures for the Mexica. For example, to rational-

ize potential consequences of weaning from mother's milk to vegetable products, she leaps to Seneca male warrior's dreams (pp. 184–86) and infers infantilism, oral gratification, passive dependency, and, ultimately, once more sacrifice and cannibalism. This was perhaps so, but one wearies of the generalizations, hyperbole, and impenetrable prose. This book, surely intended for the fanfare of 1992 and a Book-of-the-Month Club readership, is just too well said.

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ROBERT HASKETT. *Indigenous Rulers: An Ethnohistory of Town Government in Colonial Cuernavaca*. Albuquerque: University of New Mexico Press. 1991. Pp. xii, 294. Cloth \$37.50, paper \$17.50.

The impression often given by scholars is that the Spanish impulse to Hispanicize the conquered peoples of Mexico led to the elimination of preconquest political organization. In fact, as this interesting book by Robert Haskett reveals, much of traditional Indian governing systems in the Cuernavaca region survived more or less intact and continued to function effectively until Mexico's independence from Spain. This conclusion differs from that of Charles Gibson (*Spain in America* [1966]), who believed that by 1590 Indian *cabildos* (town councils) existed "principally to collect tribute and to dispense minor punishments" (Haskett, p. 3). The area Haskett studies, however, was part of the Marquesado del Valle, the great estate belonging to Fernando Cortés and his heirs. Consequently, Spanish administration of the region was itself distinct, which may have been true for the postconquest indigenous administration as well. For example, even though towns of the Cuernavaca region were like others in that they were required to pay tribute (to the Marqués) and to provide labor for the *repartimiento*, no Spanish municipality existed in the area, leaving town governments to the Indians.

The region had been invaded by the expansionist Aztec triple alliance in the mid-fifteenth century and its people forced to pay tribute to them. Despite the presence of a Mexican garrison and officials, who significantly diminished the independence of Cuernavaca, the region retained its traditional form of local government to a considerable degree and maintained a good measure of autonomy. Moreover, occupation by these outsiders some eighty years prior to the arrival of the Spanish conquerors conditioned the residents of the area to change and to the disruption of their political system. Under European rule they of course had even more dislocations imposed on them. Haskett discusses the ways in which Indians met these changes while trying to preserve their traditions in a valuable chapter on "Coping with the Cultural Conquest." Even though more informal Spanish innovations of a social nature, such as the various religious

holidays complete with fireworks, ceremonies, processions, and feasting, were popular and officially sanctioned, Spanish officials tried unsuccessfully to discourage the consumption of *pulque* and the staging of bullfights.

Even the more serious concerns of self-government were enforced indifferently. With reference to governing elections, in Indian towns the laws were often disregarded or met only partially so that some customary native practices were preserved. Haskett explains the electoral process and the operation of town government. He expands on official duties of Indian leaders along with a discussion of their society and culture and how they changed over time. Other chapters cover their career patterns, social status, and ethnicity, as well as marriage and the family. The book's value is enhanced by its many tables showing personal data by officer type, names, and terms of individual *caciques*. This is an important work based to great extent on original documents, many in Nahuatl. The result significantly expands our knowledge of Indian government under Spanish rule.

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LOUISA SCHELL HOBERMAN. *Mexico's Merchant Elite, 1590–1660: Silver, State, and Society*. Durham, N.C.: Duke University Press. 1991. Pp. xiv, 352. \$39.95.

Carefully crafted studies about seventeenth-century Mexico are as overdue as they are welcome. Louisa Schell Hoberman's study of Mexico's merchant elite moves toward providing a more balanced overview of Spain's most important overseas colony. Through such works the previously elusive seventeenth century, when Spain and Mexico refined and institutionalized their administrative structures and relationships, takes on clearer shape and substance. And while silver, state, and society represent substance, it is the shape of economic, political, and social influence exercised by the merchants that concern the author. Her far-ranging and ambitious study challenges basic assumptions about the transfer of European institutions, and their theoretical underpinnings, across the Atlantic. Her findings, she insists, call "for a theoretical framework which recognizes the colonial merchant as a distinct type" (p. 276). And while framing her data about the silver traders in a dependency and world-systems analysis, she finds both "traditional techniques" and "modern elements," not a metropole element that can "be dismissed as a parasite on the peripheral economies" (p. 276).

Although their numbers fluctuated from roughly 700 at the beginning of 1600s to one-third that number by the end of the 1630s, Mexican merchants dominated and significantly controlled both the Atlantic and Pacific trade. The merchants are divided,



based on value of merchandise shipped, into large, medium, and small shipper categories, with a small monopolistic group of large shippers controlling between 60 and 70 percent of Mexico's overseas trade. Most were born in Spain, maintained familial commercial linkages in Andalusian ports while operating out of Mexico City, and managed to accumulate wealth and status through systematic long-term rather than spectacular profit-taking activities. Although profits were considerably lower than earlier studies have suggested—in the 30–70 percent range, rather than the 100–200 percent usually cited—they greatly exceeded colonial returns in lending, agriculture, textile production, or urban real estate. A merchant's career normally followed a regular course: a period of apprenticeship in a family business and a junior partnership of considerable duration; then the status of independent trader once the merchant acquired a well-dowered wife and Mexico City residence; finally, with expanded business activities and purchase of public office, the merchant eventually became a public figure of status and influence. En route, merchants mastered the subtleties of give-and-take in relationships with royal officialdom and creole economic players in the colony: increased convoy taxes were countered by more smuggling of silver; loans to the frequently strapped colonial treasury were offset by influences reducing taxes; creole mining and agrarian ventures brought forth rich dowries from marriageable daughters; and the risks inherent in transoceanic trade produced wealth, status, and control of trade.

Basing her study on a wide range of archival documents from both sides of the Atlantic and possessing a firm grasp of published scholarship, Hoberman presents her portrait of Mexico's merchant elite in six detailed chapters. She begins with a comprehensive overview of the dominating features of the colonial context, which is followed by three chapters that describe the formation of the merchant elite, how merchant capital impacted Mexico's mining and silver mint activities, and merchant involvement in agriculture, manufacturing, and urban real estate. Three additional chapters focus on influence and interactions with public and private sectors in terms of public office and private gain, the politics of trade and taxes, and the merchants' capacity to accumulate property and produce progeny. These chapters, crammed with monetary minutiae, at times distract from the overall picture. For colonial scholars long accustomed to the lack of detail from the period covered, however, this abundance of detail—the accumulation of data extracted by the author from her far-flung sources is exceptional—produces a rich mine of information about the merchant sector.

In the conclusion of this excellent monograph, Hoberman summarizes her findings and skillfully links them to questions posed about the seventeenth century. Although the economy of New Spain contracted between the late 1630s and the 1660s, it was

not a century of depression. The history of the merchant elite demonstrates fluctuations and continuity in the export trade. While the first and last thirds of the century were dynamic, and the middle third spelled ruin and depression for some regions and wholesalers, there was little "change in the fundamental characteristics of the commercial elite" or "the export orientation of the economy" (p. 264). And contrary to the three-generational wealth-to-poverty cycles found in other sectors of colonial Mexican life, the merchants successfully established the basis for a progressive accumulation of wealth and status. The launching-pad was the silver trade, but intermarriage and expanded commercial activities produced a more stable colonial foothold. Children and grandchildren did well, accumulating civil and ecclesiastical titles. By 1660, Mexico's merchant elite was a dominant commercial force and "as wealthy or wealthier than the most respected landowner or bureaucratic families" (p. 223).

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DONALD FITHIAN STEVENS. *Origins of Instability in Early Republican Mexico*. Durham, N.C.: Duke University Press. 1991. Pp. xiv, 184. \$29.95.

Students of nineteenth-century Mexican history must confront the vexing issue of Mexico's decline from apparent colonial prosperity and stability to seemingly perpetual economic crisis and political instability in the post-independence period. Modern commentators and scholars, as well as contemporaries, have proposed various reasons for the disastrous half-century following independence. In this revised dissertation, Donald Fithian Stevens tests "the major explanations for Mexican instability proposed over the last several decades in the historical and qualitative social science literature" (p. 5) by applying statistical analysis.

In successive chapters the author examines the relationship between annual variation in economic fluctuations, fiscal crises, and political instability; the hypothesis that there was a thread of consistency in the struggles from the time of independence to the end of the wars of the Reform and the French intervention; explanations based on correlations between ideology and the occupations and careers of the political elite; the effects of geographic origins, training, experience, and political positions on the durability of presidents and cabinet ministers; hypotheses that suggest a relationship between regional social structures and the origins of political factionalism; and social stratification and the households of elite politicians in Mexico City. The study includes extensive supporting tables and appendices, one of which explains the statistical tools used.

While Stevens's analysis tends to confirm some commonly held beliefs, such as the geographical

location of conservative and liberal strength, others are refuted, like Justo Sierra's "when salaries are paid, revolutions fade" (p. 10). Stevens asserts that "the time has come to move beyond the myth which blames the crisis of early republican Mexico on the personalities of politicians, economic crises, and short term fiscal fluctuations" (p. 27). And he concludes that "Centuries of social manipulation by the monarchy made an orderly transition to republican institutions impossible" (p. 115).

Gaps in the documentation on which a study like this must rely impose limitations on the author who must make assumptions as the analysis progresses; Stevens is careful to acknowledge the limitations of his study. This slim volume is rich in information, carefully crafted, and suggestive and persuasive in its arguments. It should provoke extensive and illuminating debate among historians of Mexico, as well as a reexamination of our convictions about the reasons for post-independence Mexican instability.

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SHELDON B. LISS. *Radical Thought in Central America*. (Latin American Perspectives Series.) Boulder, Colo.: Westview. 1991. Pp. xi, 290. Cloth \$45.00, paper \$16.95.

This is a book about radical intellectual activists in Central America, the majority of whom became radicalized during the 1930s and 1940s and reached maturity in the 1950s and 1960s. They belong to the new postliberal generation that guided the thinking of the Guatemalan and Nicaraguan guerrillas during the 1960s and 1970s, and the Salvadoran guerrillas in the 1970s and 1980s. This was the generation of thinkers, most of them Marxists of one kind or another, who came under the influence of Antonio Gramsci and José Carlos Mariátegui, and who were inspired by the legendary exploits of Augusto César Sandino in Nicaragua and Agustín Farabundo Martí in El Salvador.

Sheldon B. Liss classifies these Marxist thinkers into three groups: rigid, or dogmatic, Marxists; critical Marxists; and Christian Marxists influenced by liberation theology. Under the heading of "Marxists"—Liss uses this term interchangeably with "socialists"—he includes the intellectuals in Central America's communist and socialist parties, along with Trotskyists, Maoists, Guevarists, Castroites, and others who have combined features of all these groups with aspects of Indianism, nationalism, anti-imperialism, and liberation theology. Thus, in line with those thinkers who have interpreted and implemented Marx's thought in diverse ways, Liss relies on a pragmatic conception of Marxism as opposed to a sectarian definition.

A companion volume to his *Marxist Thought in Latin*

*America* (1984), which does not include Central America or the thinking of a fourth category of non-Marxist radical intellectuals, this book supplements its internal analysis of ideologies with an external examination of the social conditions to which they responded and their influence in challenging and changing bourgeois institutions. While the work focuses on political and social topics, covering most of the concerns of radical intellectuals in Central America, it is also a contribution to intellectual history. Although not a history of recent social and political events, it provides historical and historiographical background material vital to an understanding of those events.

As a survey of the leading contemporary radical thinkers in Central America, Liss's book has no peers. With a few thorny exceptions, he clearly conveys these thinkers' ideas and their bearing on the present crisis in Central America. Among the exceptions, Liss's single reference to Stalinist influence in the region will lead most readers to conclude that it was virtually nil. He simultaneously exaggerates the influence of Trotsky on Farabundo Martí and the contemporary Sandinistas. Víctor Raúl Haya de la Torre, founder of the American Popular Revolutionary Alliance (APRA), is abruptly dismissed as a "liberal reformist," despite his professed Marxism and support of the Communist International in the mid-1920s. Liss claims that Lenin included the intelligentsia as part of the proletariat, despite Lenin's agreement with Karl Kautsky that a socialist ideology can only be impressed on the proletariat from outside by intellectuals who are not wage earners. That Sandino was a "populist revolutionary liberal" may be accurate as far as it goes, but it overlooks the essential ingredient in Sandino's description of himself as a "communist." Liss acknowledges the margin of error in endeavoring to portray the *pensadores'* radical ideas with precision. His work therefore is to be commended for its courage.

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ROBERT COHEN. *Jews in Another Environment: Surinam in the Second Half of the Eighteenth Century*. (Brill's Series in Jewish Studies, number 1.) New York: E. J. Brill. 1991. Pp. xv, 350. \$76.92.

In 1788, David Nassy, a descendant of Portuguese Jews who lived out his life on "the wild coast," wrote *Essai historique sur la colonie de Surinam, sa fondation, ses révolutions, ses progrès, depuis son origine jusqu'à nos jours; avec l'histoire de la nation juive portugaise et allemande y établie, leurs privilèges*. For 200 years, the *Essai* provided most of what we knew about the Jews of Surinam.

Taking Nassy as his point of departure, but moving forward in time, Robert Cohen develops our understanding of the community—or, more precisely, communities—by setting them within an environmental

framework. Having consulted archives in the Netherlands, Surinam, and the United States, he sifts his documentation through the statistician's sieve and comes up with some interesting conclusions. A few of these corroborate Nassy's observations, but more upset them. Nassy was writing at the end of a prosperous period, looking back on the "good old days" with nostalgia. Cohen examines the bleaker period that followed.

For a while, the Jews of Surinam were among the most privileged in the world. Planters gathered around their own municipality, the Joden Savanna, struggled with the forces of nature, and reaped munificent rewards in the world sugar market. The slave economy that sustained all of Surinam at this date provided the base for a social pyramid that included a tiny middle class and a white apex to which Jews did not quite belong but from which they were not totally excluded. By the second half of the century things had changed. The plantation economy declined, and the arrival of Ashkenazi merchants in Paramaribo shifted the communal balance of power from the countryside to the city. In this book, the resultant rivalry among religious leaders becomes one characteristic among many to be considered, rather than occupying center stage, as in the work of Isaac Emmanuel and Suzanne Emmanuel (*History of the Jews of the Netherlands Antilles*, 2 vols. [1970]).

Following a brief chapter in which he defines his terms, Cohen analyzes five "environments." Statistical analysis of migration and mobility reveals a shift in the type of person who came out to the colony: no longer investors in plantations and mills, but the unwanted Jewish poor of Amsterdam, dumped in Paramaribo as a sort of workfare measure. Climate took its toll throughout the European population, more so among the newly arrived, the poor, and the newborn. In one interesting aside, Cohen uses Jewish mortality statistics to disprove the observation of contemporaries that women had greater immunity to epidemics than did men. He tests Lawrence Stone's "low gradient affect" (disregard for the death of children) against the keen of a Jewish father on the death of his daughter. Relations between white Sephardim and their mulatto offspring are described by analysis of congregational rules set down for the latter's behavior—rules that were enforced by the government. The fluctuating economy and the cultural environment also come in for analysis. Here, Nassy himself becomes a subject of the text, for Cohen presents an inventory of his library. Comparing it with the top 100 best sellers in Paris of that epoch, he finds the planter-apothecary-merchant-physician-historian to have been as well-read as any French intellectual.

Cohen revived the study of Surinam and its unique communities with his earlier essays on demography, the export of the impoverished to the Dutch colonies, and patterns of marriage among the Sephardim. His well-edited *Jewish Nation in Surinam*:

*Historical Essays* (1982) is a worthy companion to the present work. Cohen's death at age forty-five has deprived us of the body of mature work we might have expected from this fine scholar.

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JOHN W. F. DULLES. *Carlos Lacerda, Brazilian Crusader*. Volume 1, *The Years 1914–1960*. Austin: University of Texas Press. 1991. Pp. xvi, 495. \$40.00.

Firebrand, lightning rod, monster, deposer of presidents, madman—these are but a few of the printable epithets hurled at Carlos Lacerda during his tumultuous career. His exceedingly tortuous political life, against the backdrop of a complicated personal story, make him a major challenge for any biographer. Luckily, he himself chose a man equal to the job, John W. F. Dulles, who has previously given us a fine treatment of Getúlio Vargas. Dulles has worked on this biography for many years, interviewing family members and associates, as well as poring over private papers, oral histories, newspapers, and secondary works. From the standpoint of documentation alone, the book is a tour de force.

The first of this two-volume project takes Lacerda from birth through his election in 1960 to the highest public office he attained, governor of Guanabara. The story itself is kaleidoscopic, dizzying, and rich. In Dulles's hands the biography becomes a diary of the national political elite from the standpoint of their most incisive critic. Ever since winning his first election in 1947, Lacerda oriented himself toward the center of the political stage, interpreting and judging others and trying to approach it himself. He founded an opposition newspaper and steered it like a juggernaut, plowing through politicians, scandals, parties, cliques, and alliances. Never a man to work in a team, he hovered about the right wing of the União Democrática Nacional and attacked virtually everyone and everything that did not meet his impossibly high standards.

Lacerda was a man driven, able to campaign for days with little rest and capable of ruining his own health in order to execute an attack. Much of the career traced here seems negatively inspired, because Lacerda struggled against what he regarded as Brazil's greatest enemy, an oligarchy that dated from the 1930 revolution. He hated it, fought it, denounced it, almost died at its hands, and eventually helped bring about its subjugation by the military in 1964.

Volume 2 promises to chronicle Lacerda's career as governor, unquestionably his most successful period. These were positive years, because he spent them rebuilding and improving the city of his childhood, Rio de Janeiro.

Dulles knows national politics during the Lacerda

years better than almost anyone, and he not only relates the events and personalities in understandable fashion but he also makes them read like a novel. To say this means a great deal, because Brazil's politics were among the most complicated, convoluted, and impenetrable of any in the world. Simply to convey them comprehensibly is a major contribution. Episodes like the 1955 Lott coup and the nomination and election of Jânio Quadros in 1960, in particular, come off more clearly than anywhere else in print. Another valuable feature is the detailed description of politics from the point of view of the practitioners, with attention to media use, image-making, party organization, and balloting.

The interpretive passages are spare, for Dulles prefers to let Lacerda and others analyze their own lives and situations. He occasionally remarks that Lacerda distorted the record, and he often provides contrary points of view. Still, Dulles is an unobtrusive judge, and we are left with an admiring portrait by one who respected his subject a great deal. Readers expecting theoretical treatments of such questions as charisma, populism, political culture, or class alliances will be disappointed.

The personal side of Lacerda, potentially fascinating, emerges slowly and tentatively, as evidence of a troubled man accumulates. We can hope that in volume 2 Dulles gives us more insights into Lacerda's emotional life, especially during his last years, which were tinged with despair and self-doubt.

Overall, this is a powerhouse biography of a man whose career marked Brazil deeply during the 1950s and 1960s. I for one anxiously await the concluding volume.

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GEORGE REID ANDREWS. *Blacks and Whites in São Paulo, Brazil 1888–1988*. Madison: University of Wisconsin Press. 1991. Pp. xiii, 369. Cloth \$47.50, paper \$17.50.

If you are not confused you do not understand. This old adage applies particularly well to Brazilian race relations. For over one hundred years, Brazilians have emphatically denied having racial prejudices or biases. Blacks as well as whites have seemingly accepted racial democracy. But since the emancipation of slaves in 1888, succeeding generations of black Brazilians have experienced racism that has blocked their social, economic, and political advancement. Far more insidious than the overt racism found in the United States or South Africa, the Brazilian variety silently kept most black Brazilians in their place.

Using the state of São Paulo as a base, George Reid Andrews attempts to explain why, throughout the twentieth century, Brazilians of all races and classes accepted the notion of racial democracy while at the same time they knew it did not exist. For Andrews, since 1888 racial inequality in São Paulo has been a

basic fact of life. Following the signing of the "Golden Law," black (*preto*) and brown (*pardo*) workers lost agricultural jobs to whites, and subsequently faced discrimination in the rising industrial sector of São Paulo.

Andrews places much of the blame for racism in São Paulo on the state apparatus. From 1888 to the 1920s, state labor policies favored whitening the work force, to the detriment of blacks. When white-collar positions opened up during the 1930s, government actions maintained blacks at menial industrial and agricultural tasks, while whites monopolized the prestigious professional and administrative positions that opened under the *Estado Novo*. Educated middle-class blacks could not compete successfully for the new, better paying white-collar jobs. Together with the private sector, the government of São Paulo also helped to perpetuate the stereotypes of black inferiority, which ascribed to blacks negative characteristics such as laziness, shiftiness, and criminal inclinations.

Much of Andrews's argument derives from a battery of evidence compiled by scholars since the 1950s. To this data he has added his own reading of the *paulista* press along with information he gathered from a number of interviews. His conclusion that racial democracy never existed in Brazil will come as no surprise to most readers. But he does trace in detail the role racism played in shaping São Paulo's black population into a largely submissive group. He also spells out why blacks have failed to challenge the very racism that kept them in their place.

As Andrews points out, starting with participation in antislavery movements and then moving through the efforts of the Brazilian Black Front to those of contemporary black movements, blacks have tried to address many of the issues that resulted from their racial condition. From the outset, black leaders wanted to improve the status of Afro-Brazilians as a group. But on every occasion they met with resistance from whites. The latter, as employers, politicians, and academics, hid their racism behind the myth of racial democracy. In so doing, they effectively kept blacks from organizing along racial lines. In a democracy, they argued, racial groups need not form political parties or movements. As Andrews somewhat sarcastically comments, whites could and did organize racial alliances, but they did not want blacks to do the same.

To understand racism in Brazil necessitates studying its roots. Why did white elites persist in their racist beliefs, while they refused to acknowledge their prejudices? Andrews clearly did not set out to answer this question. He adequately summarizes the decline of racial democracy as a viable policy in São Paulo, for blacks as well as whites. But the next step involves studies of white elites, their attitudes, their motives, and their racial politics, all of which have kept racism alive and well in São Paulo for over a century.

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## Collected Essays

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These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

JAY O'BRIEN and WILLIAM ROSEBERRY, editors. *Golden Ages, Dark Ages: Imagining the Past in Anthropology and History*. Berkeley and Los Angeles: University of California Press. 1991. Pp. x, 288. \$35.00.

WILLIAM ROSEBERRY and JAY O'BRIEN, Introduction. WILLIAM ROSEBERRY, Potatoes, Sacks, and Enclosures in Early Modern England. HERMANN REBEL, Reimagining the *Oikos*: Austrian Cameralism in Its Social Formation. MICHAEL PAINTER, Re-Creating Peasant Economy in Southern Peru. LOUISE D. LENNIHAN, Custom and Wage Conflict: Problems of Periodization and Chronology in Northern Nigerian Labor History. JAY O'BRIEN, Toward a Reconstitution of Ethnicity: Capitalist Expansion and Cultural Dynamics in Sudan. JAY SPAULDING and LIDWIEN CAPTEIJNS, The Orientalist Paradigm in the Historiography of the Late Precolonial Sudan. TED SWEDENBURG, Popular Memory and the Palestinian National Past. GAVIN SMITH, The Production of Culture in Local Rebellion. GERALD M. SIDER, House and History at the Margins of Life: Domination, Domesticity, Ethnicity, and the Construction of Ethnohistories in *The Land God Gave to Cain*. MICAELA DI LEONARDO, Habits of the Cumbered Heart: Ethnic Community and Women's Culture as American Invented Traditions.

KARIN PENDLE, editor. *Women and Music: A History*. Bloomington: Indiana University Press. 1991. Pp. x, 358. \$27.50.

ANN N. MICHELINI, Women and Music in Greece and Rome. J. MICHELE EDWARDS, Women in Music to ca. 1450. KARIN PENDLE, Women in Music, ca. 1450–1600. BARBARA GARVEY JACKSON, Musical Women of the Seventeenth and Eighteenth Centuries. NANCY B. REICH, European Composers and Musicians, ca. 1800–1890. MARCIA J. CITRON, European Composers and Musicians, 1880–1918. ADRIENNE FRIED BLOCK and NANCY STEWART, Women in American Music, 1800–1918. CATHERINE ROMA, Contemporary British Composers. ROBERT ZIEROLF, Composers of Modern Europe, Australia, and New Zealand. J. MICHELE EDWARDS and LESLIE LASSETTER, North America since 1920. S. KAY HOKE, American Popular Music. MICHAEL J. BUDDS, African-American Women in Blues and Jazz. LINDA WHITESITT, Women's Support and Encouragement of Music and Musicians. L. JAFRAN JONES, Women in Non-Western Music.

RENÉE COX, *Recovering Jouissance: An Introduction to Feminist Musical Aesthetics*.

JUDITH R. BASKIN, editor. *Jewish Women in Historical Perspective*. Detroit: Wayne State University Press. 1991. Pp. 300. Cloth \$39.95, paper \$19.95.

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# Other Books Received

The following books were recently received in the AHR office. Books listed here do not include works scheduled for review.

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- ACKERMAN, BRUCE. *The Future of Liberal Revolution*. New Haven: Yale University Press. 1992. Pp. viii, 152. \$18.50.
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- GRIGG, DAVID. *The Transformation of Agriculture in the West*. (New Perspectives on the Past.) Cambridge, Mass.: Basil Blackwell. 1992. Pp. xiv, 141. \$19.95.
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## ARTICLES

### TO THE EDITOR:

The admirable survey of historiography in Czechoslovakia by Jiří Kořalka [*AHR*, 97 (October 1992): 1026–40] describes a middle, “gray zone” of research that survived in the 1970s and 1980s after the Warsaw Pact invasion in August 1968 led to a severe repression of historians. In that “zone,” Kořalka writes, some historians maintained a connection to their fields and even published in “foreign countries” (p. 1034). The author appropriately might have mentioned at that point the role of the journal *East Central Europe* in soliciting and publishing articles and reviews by such historians as Jaroslava Hoffmannová, Arnošt Klíma, Pavel Krivský, Josef Macek, Jiří Rak, and Zdeněk Šolle, several of whom are cited as “gray zoners” by Kořalka. Czech historians have told me that no foreign country concentrated so objectively, in those oppressive years, on the history of Czechoslovakia as the United States, and in particular, that journal. A fruitful program of exchange of Kořalka's own journal, *Husitský Tábor*, with American university libraries, instituted in the early 1980s, also deserved mention as a sign of such outreach.

STANLEY B. WINTERS  
*New Jersey Institute of Technology*

### JIRÍ KOŘALKA REPLIES:

Stanley B. Winters is right in his comment on my article. I regret not having mentioned in detail the substantial support and help that the independent Czech scholars received before 1989 from many colleagues and friends in the United States, Germany,

Austria, and other countries. Besides the *Österreichische Osthefte* in Vienna, the journal *East Central Europe* (where Professor Winters was responsible for the Czechoslovak issues) was the foremost periodical that regularly published articles and reviews by Czech historians outside the official camp. The commemorative volume on Arnošt Klíma and Pavel Krivský in this journal is of lasting value.

JIRÍ KOŘALKA  
*Prague*

## REVIEWS OF BOOKS

### TO THE EDITOR:

Given that most of your readers are unlikely to follow the historical debates in Denmark or do not read Danish, I feel compelled to comment on H. Arnold Barton's review [*AHR*, 97 (October 1992): 1231–32] of Thorkild Kjærgaard's doctoral dissertation, *Den danske revolution 1500–1800: En økohistorisk tolkning*.

Although I agree in general with Barton's balanced review, I do not believe that his laudatory concluding paragraph is justified. In omitting to mention the serious flaws in use of sources, chronology, and interpretation, which were revealed in the debate following the publication of *Den danske revolution* in Denmark in December of 1991, Barton leaves your readers with the impression that Kjærgaard's approach and results represent a major international breakthrough. (The debate, including the contributions of the two “official opponents” at the public defense of the dissertation, is published in the two Danish journals *Historisk Tidsskrift* [D] 1992, 1, and *Fortid og nutid* 1992, 1.)

The book is a bold attempt at a multidisciplinary interpretation. Regrettably, Kjærgaard's methodology and conclusions have been the subject of weighty criticism by generalists as well as by experts in virtually all the disciplines he has touched on. Individually, these criticisms may seem picayune, given the ambition and sweep of the book; collectively, they raise serious doubts about the viability of Kjærgaard's approach. Even though most of the experts (and this

writer) agree with Barton that "Danish historiography [of the agrarian reforms of the eighteenth century] will surely never be the same," *Den danske revolution* ultimately fails to fulfill its promise. Kjærgaard has challenged every future historian of eighteenth-century Denmark to respond to his brilliant but unproven hypothesis, either by documenting its viability or by disproving it. This is the import of *Den danske revolution*, no more and no less.

JOHN P. MAARBJERG  
Yale University

H. ARNOLD BARTON REPLIES:

I am not sure just how far apart John Maarbjerg and I actually may be regarding Thorkild Kjærgaard's book. I appreciate that he considers my review of it a balanced one. It is clear that such a "bold attempt at a multidisciplinary interpretation," in his words, is bound to raise significant questions among general-

ists, and I have pointed to some myself. To what degree, however, the specific, detailed criticisms raised by specialists—which could hardly be avoided in a work of such "ambition and sweep"—taken together call into doubt the "viability of Kjærgaard's approach" must remain a matter of individual judgment. I hold to my view that Kjærgaard has broken significant new ground for international historical scholarship and has done so brilliantly. In my opinion, he has supported his thesis in impressive fashion. I, meanwhile, agree entirely with Maarbjerg's assertion that Kjærgaard has challenged all who henceforward work in that period of Danish—and I would add, European—history to respond to his pioneering study, which is ever the proof of a historical work of unusual breadth and vision.

H. ARNOLD BARTON  
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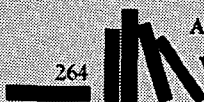
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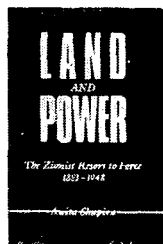
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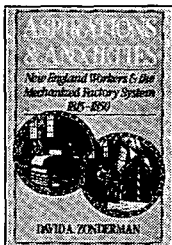
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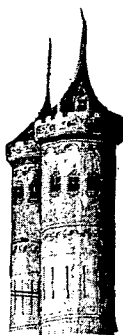
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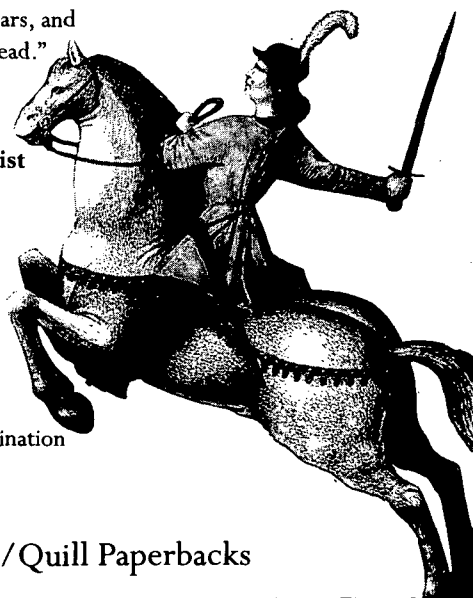
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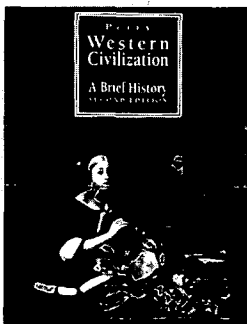


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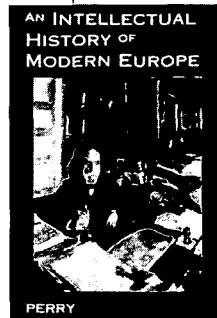
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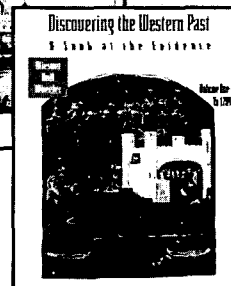
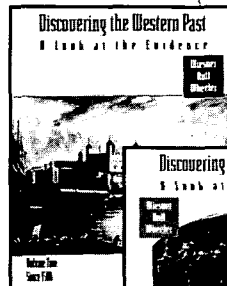
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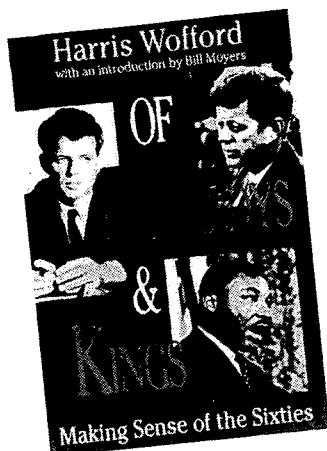
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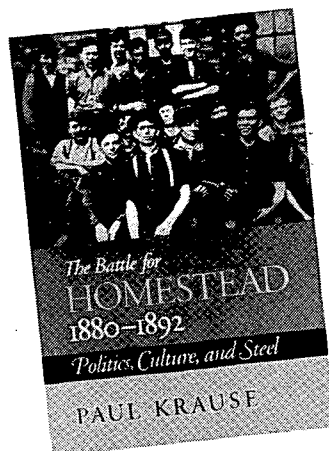
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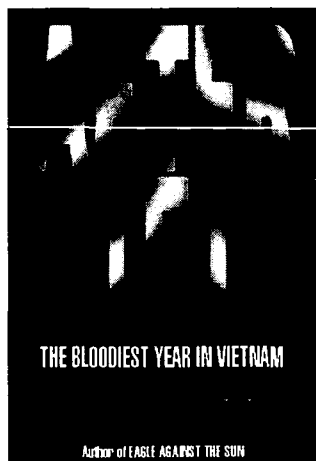
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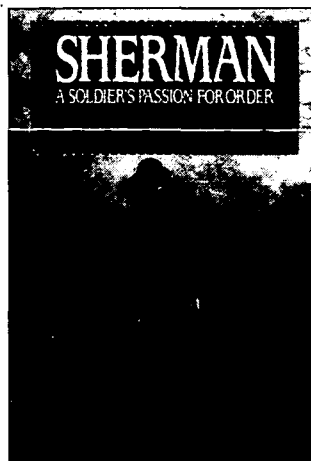
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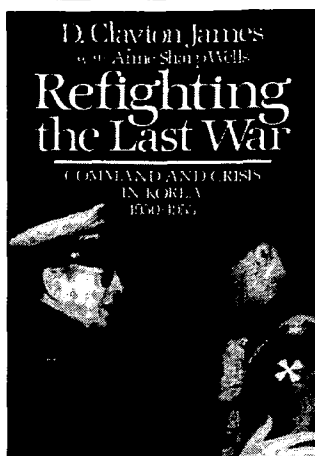
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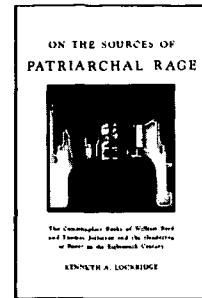
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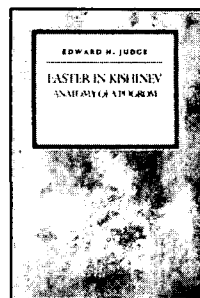
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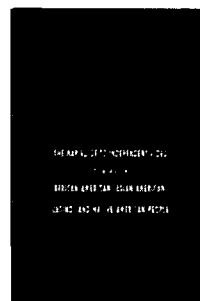
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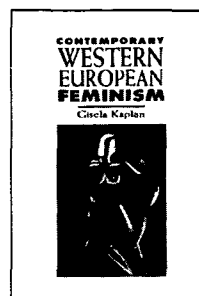
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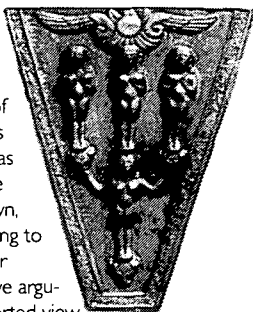
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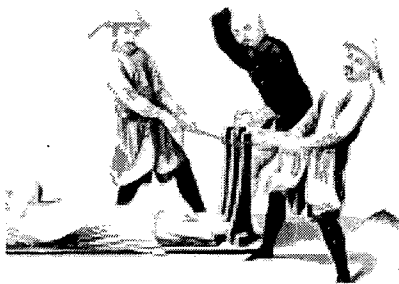
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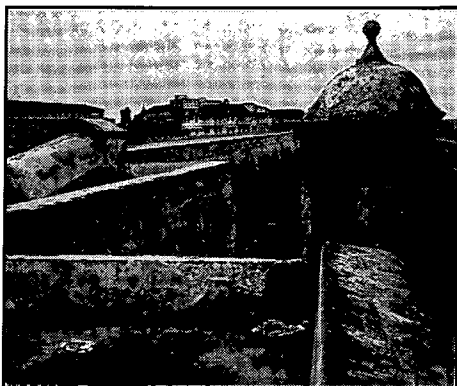
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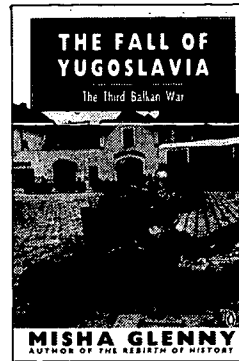
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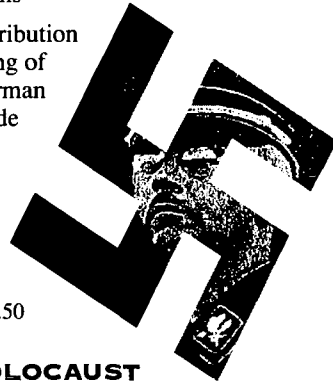
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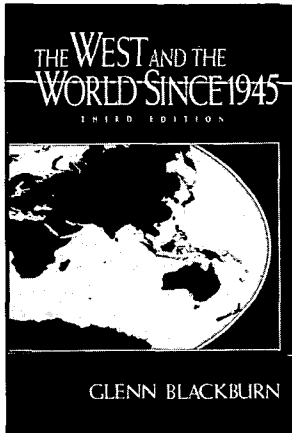
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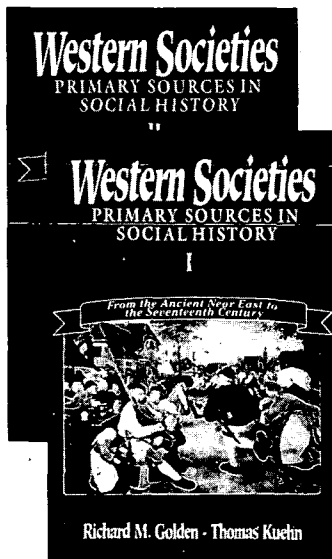


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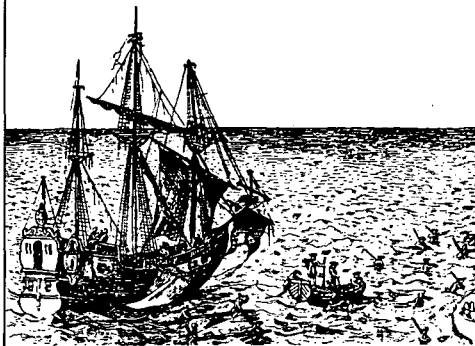
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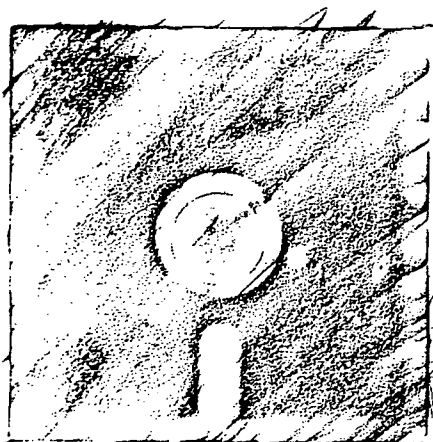
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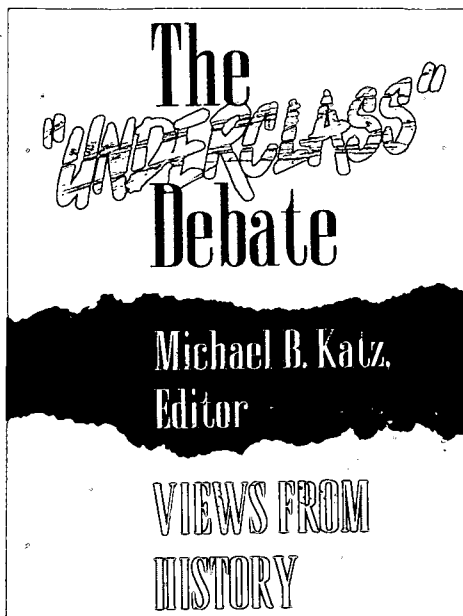
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